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OF THE

SOCIETY OF TELEGRAPH ENGINEERS,

INCLUDING

ORIGINAL COMMUNICATIONS ON TELEGRAPHY AND ELECTRICAL SCIENCE.

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SOCIETY OF TELEGRAPH ENGINEERS.

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The Thirty-first Ordinary General Meeting was held on Wednesday, the 13th January, 1875, Mr. LATIMER CLARK, President, in the Chair.

The President read his Inaugural Address:—

GENTLEMEN,

On assuming the Chair as President of the Society of Telegraph Engineers I desire to return you my thanks for the confidence you have shown and the honour you have done me in placing me in so responsible a position. Although I shall spare no effort to uphold the interests and dignity of the Society and to increase its utility, and although I shall fulfil these duties as a labour of love, I am conscious that I shall have to ask your continued forbearance and indulgence for deficiencies.

My task however will be but a light one compared with that which devolved on my predecessors in this chair. They had to create and establish the Society and to perfect its organisation; and, thanks to their wise and skilful guidance and to the liberal assistance of the Institution of Civil Engineers, we are able to meet in this magnificent room this evening some 600 members strong, and to chronicle nothing but prosperity and progress.

In this respect indeed we have ground for sincere congratulation, for even the parent Society, the Institution of Civil Engineers, one of the most prosperous and flourishing institutions in the world, was, I believe, thirty years in existence before it possessed as many

members as we now number in our single branch of the profession.

The Electric Telegraph is not the creation of any one mind, nor did it come upon the world suddenly or in a complete form: but, like the pale beams of the rising moon, its dawn may be traced back for generations, continually brightening and gaining strength as fresh additions to our knowledge rendered the problem more feasible.

Practical telegraphy is however a creation of the present generation. Within the memory of most of those now present it has burst into existence and spread throughout the world with a celerity and splendour characteristic of the mysterious element which it has bent to its service, so that to-day, from the distant shores of the Pacific Ocean to the eastern limits of Asia, not only every land but almost every town and village has been united by the electric wire, and distance and time have been so changed to our imaginations, that the globe has been practically reduced in magnitude, and there can be no doubt that our conception of its dimensions is entirely different to that held by our forefathers.

It is impossible to avoid looking back and inquiring what has been the cause of this singular development; and, although the ground has been trodden so often, I cannot refrain from passing in hasty review some of the steps in the rise and progress of telegraphy. For, though the history of our art may receive but little attention from the world at large, it must ever possess an interest for the members of this Society, many of whom have borne so prominent a share in its creation and development.

There was a fabulous story among the old authors that two needles touched by the same magnet and suspended within an alphabetic circle would move in unison at whatever distance they might be separated. The most interesting form of this story is that related by Joseph Glanvill, M.A., in his *Scepsis Scientifica*, published in 1665. He says, "That men should confer at very distant removes by an extemporary intercourse is another reputed impossibility; but yet there are some hints in natural operations that give us probability that it is feasible and may be compassed without

unwarrantable correspondence with the people of the air. That a couple of needles equally touched by the same magnet being set in two dyals exactly proportioned to each other and circumscribed by the letters of the alphabet may affect this magnate hath considerable authorities to vouch for it. * * * * Now, though this pretty contrivance may not yet answer the expectation of inquisitive experiment, yet it is no despicable item that by some other such way of magnetic efficiency it may hereafter with success be attempted when magical history shall be enlarged by riper inspections, and it is not unlikely but that present discoveries might be improved to the performance."

Now here we have the early dawn of the idea of an electric telegraph.

On the 1st of February, 1758, a Scotchman, Charles Marshall, of Paisley, then resident at Renfrew, published in the "Scots Magazine" a full and clear description of a practicable electric telegraph, and suggested the coating of his wires with an insulating material; and he may be therefore considered, in a sense, the inventor of the telegraph.

In 1800 Galvani and Volta introduced the voltaic pile, which forms so important a feature of the telegraph of to-day.

From this time many other forms of telegraph were proposed which it is unnecessary to notice, except that in 1809 Dr. Sœmmering laid before the Academy of Science at Munich a plan in which for the first time the galvanic battery was employed for the transmission of the current and the decomposition of water. He also expressed the hope that his system might serve to telegraph between Munich and Augsburg, and took much pains to make it known.

In 1816 our late lamented member Sir Francis Ronalds produced his electric telegraph, and at great expense and trouble erected a considerable length in his garden at Hammersmith. He employed frictional electricity and only one wire, and exhibited his signals by the divergence of pith balls, combined with rotating dials working synchronously, a system afterwards brought to great perfection in the printing telegraph of Professor Hughes. Sir

Francis Ronalds will always take a high position in the history of the telegraph, not so much on account of the excellence or originality of his invention, as on account of the confidence and ardour with which he pursued his experiments and endeavored to bring them to the notice of his countrymen. With wonderful prevision he fully perceived its value and foretold its destiny. His "Description of an Electrical Telegraph" which was published in 1823, the first book ever published on the subject of electric telegraphy, might almost serve for a description of a telegraphic system at the present day. He proposed the establishment of telegraph offices throughout the kingdom, and pointed out the benefits which the Government would derive from their existence. He described methods of insulating the wires, either on poles or underground, with all the details of tubes, joints, and testing-boxes, testing-stations, linemen, and inspectors, as at the present day. But the most interesting and singular point to my mind is the clearness with which he foresaw and explained the phenomenon of retardation of the electric current by induction in underground wires, a phenomenon which has subsequently so greatly engaged the attention of electricians.

The influence of this is so great that on our Atlantic cables we do not transmit messages at a greater rate than fifteen or twenty words per minute, whereas, if the effects of induction could be removed, we might transmit three or four hundred words per minute. After showing that the discharges through his eight miles of insulated wire were apparently instantaneous, he says:

"Yet I do not contend, nor even admit, that an *instantaneous discharge* through a wire of unlimited extent would occur in all cases"; and recurring to the subject further on he says,

"That objection which has seemed to most of those with whom I have conversed on the subject the least obvious appears to me the most important, and therefore I begin with it, viz.: the probability that the electrical induction which would take place in a wire enclosed in glass tubes of many miles in length (the wire acting like the interior coating of a battery) might amount to the retention of a charge, or at least might destroy the suddenness of the dis-

charge, or in other words might arrive at such a degree as to retain the charge with more or less force even when the wire is brought into contact with the earth."

He then proceeds to suggest methods of obviating the difficulty or experimentally demonstrating its extent and character.

There can be no doubt that, if Ronalds had worked in the days of railways and joint stock enterprise, his energy and skill would have triumphed over every difficulty, and he would have stood forth as the practical introducer of the telegraph. But he was thirty years before his age, and the world was not ready for him.

Having completed his arrangements, he modestly invited Lord Melville on the 11th July 1816 to witness his experiments, in order that he might demonstrate the nature and merits of his invention.

The reply he eventually received was eminently characteristic of the neglect and even contempt with which science and scientific men were, and to some extent still are, regarded by statesmen.

"Mr. Barrow presents his compliments to Mr. Ronalds, and acquaints him, with reference to his note of the 3rd instant, that telegraphs of any kind are now wholly unnecessary, and that no other than the one now in use will be adopted."

Colonial Office, 5 Aug. 1816.

The inventor had chosen an unfortunate time. The great war of the century was concluded; the government officials were doubtless closing up their affairs after a weary session, and were thinking only of salmon and grouse; telegraphs and all other new-fangled ideas were wholly unnecessary, and Mr. Ronalds was probably only one amongst a dozen of inventors who received their *coup de grace* on that unlucky August morning.

Ronalds was however contending against one difficulty which, as we now know, would have been almost insuperable. Although very familiar with the galvanic pile, and although Sœmmering had used it for telegraphs seven years previously, he was still working with high tension or frictional electricity.

In 1819 Oersted discovered the effect of the current in causing a deflection of the magnetised needle, and in 1820 Ampere pro-

posed to construct a telegraph by means of the voltaic battery, the coil of wire, and the magnetised needle. This was followed by the long series of Faraday's wonderful electrical researches.

In 1827 Dr. Jacob Green, of Jefferson College, Philadelphia, wrote as follows:

"In the very early stage of electro-magnetic experiments it had been suggested that an instantaneous telegraph might be constructed by means of conjunctive wires and magnetic needles. The details of this contrivance are so obvious, and the principles upon which it is founded are so well understood, that there was only one question which could render the result doubtful. This was, whether by lengthening the conjunctive wires there would be any diminution in the electrical effect upon the needle. * * * Had it been found true that the galvanic fluid could be transmitted in a moment through a great extent of conducting wire without diminishing its magnetic effect, then no question could have been entertained as to the practicability and importance of the suggestion adverted to above with regard to the telegraph. Mr. Barlow, of the Royal Military Academy (at Woolwich), who has made a number of successful experiments and investigations in electro-magnetism, fully ascertained that there was so sensible a diminution with only 200 feet of wire as to convince him at once of the impracticability of the scheme."

There can be little doubt that this published opinion of so eminent a man as Professor Barlow, which occurs in the Philosophical Transactions, had the effect of retarding the introduction of electric telegraphs by many years.

In the same year Ohm published the celebrated mathematical formulæ which bear his name, and had they been known and duly appreciated at the time they would at once have dispelled all misgivings as to the distance at which electrical effects might be rendered sensible. They were not however translated into English until 1841.

In 1828 Green, of Nottingham, published his valuable mathematical investigations of the distribution of electricity on the surace of conductors of various forms.

In 1834 Wheatstone immortalised his name by his magnificent experiment on the velocity of electricity, and by his other researches on the subject, which doubtless caused many minds to ask themselves, as Ronalds had done, "Why has no serious trial yet been made of the qualifications of so diligent a courier?"

We now approach the memorable epoch of 1837. Scientific men were in possession of every knowledge and appliance necessary for creating a perfect electric telegraph; the subject was commonly lectured on; fresh methods of communication continued to be invented, among which I will only mention that of Baron Schilling, who in 1832 employed five wires insulated by silk and five vertical needles.

Railways had also now come into extensive use, and the world was in every way ripe and ready for the practical introduction of the telegraph. In March, 1836, Mr. Wm. F. Cooke appears to have been present at one of these public lectures, and, struck by the adaptability of the telegraph to the requirements of railway traffic and commercial use, at once made the subject his exclusive study. Returning to England on the 22nd April, he appears to have devoted the remainder of the year to the study of the subject and the perfection of his ideas, the most important feature of which appears to consist in the fact that he for the first time introduced the use of an electro-magnet for telegraphic purposes.

His first model, made out of a musical snuff-box with an electro-magnetic escapement, was made at Heidelberg. He first exhibited an instrument of this form to Professor Faraday in November 1836, and subsequently to the Directors of the Liverpool and Manchester Railway in January 1837, with a view to its adoption on the incline of the Liverpool Tunnel, which was then worked by a rope and a fixed engine. The instrument gave sixty signals, and was considered too novel and complex for the purpose required; and, before simpler instruments could be constructed, they adopted a pneumatic telegraph.

We now arrive at the epoch of 1837, the year in which the first practical telegraphs were introduced. Several electric telegraphs were invented during this year, any of which, in the absence of

others, would without doubt have laid the foundation of the practical telegraph. Among them those of Cooke, Wheatstone, Morse, and Steinheil require especial mention. The telegraph of Professor Steinheil deserves notice on account of its great ingenuity and completeness. He employed only one wire, and transmitted his signals either by sound or by an alphabet of dots printed on a strip of paper, and he employed the earth circuit. His experiments were performed on a distance of several miles between the Royal Academy at Munich and Bogenhausen, and his telegraph was certainly very far in advance of any other existing at that date. His system was not however brought into further use at that period.

In February 1837 Mr. Cooke, by the advice of Professor Faraday and Dr. Roget, made the acquaintance of Professor Wheatstone, and in June they had formed a partnership and taken out a joint patent. Much difference cf opinion has arisen as to the due apportionment of the merit of these gentlemen in connection with the invention or introduction of the electric telegraph, but it is not our purpose to-day to inquire into the merit of these respective claims. Happily abundant documentary evidence exists to enable those who take an interest in the question to form their opinions upon it.

It appears to me however that neither of those gentlemen can in any sense claim to have been the inventor of the electric telegraph. In fact, if we except the use of the electro-magnet and the mechanical escapement, I do not find in their inventions of this period any important novelty of combination or of principle which appears likely to survive in that process of "natural selection," that "struggle for existence," which goes on as persistently among the productions of art as in the province of nature.

Their claim for distinction must rest rather on the energy and success with which they introduced their system into practical use and compelled the world to recognise its merits.

By the deed of partnership executed between these gentlemen it was arranged that Mr. Cooke was to continue the entire practical management and control of their affairs, and accordingly on the

27th of June Mr. Cooke was introduced to Mr. Robert Stephenson, who at once took the greatest interest in the invention and lent it all his influence and assistance. It is gratifying to find the name of one of the fathers of the railway system thus early acting as a father to the electric telegraph.

On the 4th of July the apparatus was exhibited to Mr. Stephenson, and by the 25th their first experimental line was in operation between Euston and Camden, and was worked in the presence of Professor Wheatstone, Mr. Stephenson, Mr. Charles Fox, Mr. Brunel, and Sir Benjamin Hawes.

The system exhibited was still that of the electro-magnetic escapement and rotary dial; the needle telegraph, which has been since so intimately associated with their names, not having been yet perfected.

At the same time Professor Morse was occupied in introducing his electro-magnetic telegraph in America. This telegraph, from its exquisite simplicity, has come into universal use throughout the world and has conferred immortality on the name of its inventor. The idea had long existed in his mind and as early as 1835 he had exhibited his experiments to private friends, but there appears no published record referring to his invention earlier than a letter in the *New York Observer* of April 15, 1837, by his brother S. E. Morse, and in this letter he speaks of the invention as requiring twenty-four wires. On the 10th March a circular letter had been sent to certain collectors of customs and others desiring information with reference to telegraphic communication, and it was this circular which probably evoked the letter in question. On the 27th September, Professor Morse wrote a letter to the Secretary of the Treasury of the United States, which shows that he had allowed the subject to lie dormant, but he promised to have a complete apparatus in operation by the 1st January, 1838. His first experiment, over half a mile, was made on the 2nd October, 1837, and on the 6th October he filed a caveat in the patent office at Washington. I believe the first working telegraphic line erected in the United States was that between Washington and Baltimore, which began work in 1844.

Reverting to the progress of the telegraph in England, we find Mr. Cooke in 1837 in negotiation with the directors of the London and Birmingham Railway for laying down a telegraph from London to Birmingham, a project which did not however arrive at completion. In the following year (1838) Mr. Cooke conducted some extensive experiments at St. Katherine's Docks, in which he exhibited the telegraph in operation through 110 miles of No. 16 covered copper wire. Mr. Brunel at this period took up the matter, and a line of pipes with five wires was laid down on the Great Western Railway from Paddington to Drayton, which was afterwards extended to Slough in 1841. The total cost of this line to Drayton was 3,270*l.* 6*s.* The insulation becoming defective, the pipes were removed and sold, and poles and iron wires were erected in their place in 1841, the wire being in the first instance temporarily insulated on quills. In 1840 Mr. George Stephenson adopted it on the Blackwall Railway, which was then worked by a stationary engine and rope. In 1841 Sir Charles Fox ordered a telegraph on the Cowlairs and Glasgow Line. It was then fixed on one of the Midland tunnels, and on the Dublin and Dalkey Atmospheric Railway.

In 1842 Mr. Cooke was in negotiation with the Admiralty in reference to a line from London to Portsmouth; he also published a book termed *Telegraphic Railways on the Single Way.* In this year plans for the Norwich and Yarmouth Line were approved by Mr. George Hudson. In 1843 the telegraph was ordered for the Dalkey and Kingstown Atmospheric Railway by Mr. Samuda, and on this line galvanised iron wire was first employed. The Norwich and Yarmouth Railway, a single line, was opened in May 1844; the Northampton and Peterborough Line and the Croydon Line were completed in 1845. These were followed by the South Western Line from London to Gosport, and the South Eastern Line, the first portion executed being the Maidstone Branch.

It appears that the funds necessary for the management of the partnership were provided by Mr. Cooke, and the results were at first by no means encouraging. His account-books, which were produced in evidence during an arbitration which took place in

1841, before Marc Isambard Brunel and Professor Daniell, showed that, prior to his connection with Professor Wheatstone in February 1837, he had expended 385*l.* 8*s.* 10*d.* on his experiments; by the end of 1843 this deficit had increased to 6,232*l.* 16*s.* 1*d.*

The celebrated message which led to the capture of John Tawell, a Quaker, who had committed a murder at Slough, was sent on the 1st January, 1845, and it had a powerful influence in arousing public attention to the value and capabilities of the telegraph.

In the course of constructing the Blackwall Railway Mr. Cooke made the acquaintance of Mr. George Stephenson and Mr. George Bidder, and in 1842 Messrs. Cooke and Wheatstone inserted a series of advertisements in the *Railway Times* and other papers drawing attention to the merits of their invention. These circumstances eventually led to Mr. Cooke's introduction by Mr. Bidder to Mr. John Lewis Ricardo, M.P. afterwards for many years Chairman of the Electric Telegraph Company, a gentleman by whose energy and enterprise the Company was created and led to a high pitch of prosperity. The first interview took place on the 1st of October, 1845, and so prompt and decisive was their action that on the 17th of that month Mr. Ricardo and Mr. Bidder wrote a joint letter to Mr. Cooke accepting the terms he had proposed.

The Company was registered on the 2nd of September, 1845, and a provisional prospectus was issued shortly afterwards.

The first directors were Mr. J. L. Ricardo, Mr. Sampson Ricardo, Mr. W. F. Cooke, Mr. George Bidder, and Mr. Richard Till. An Act of Parliament was obtained on the 18th June, 1846, and they commenced business, in an imperfect manner, at their first offices at 345, Strand, where they educated their clerks, the system employed being the Cooke and Wheatstone double-needle instrument.

The capital of the Company was privately subscribed by the directors above named, and it would appear that under the arrangements made with the patentees they received about 160,000*l.* for their patents in money or value. This purchase included Mr. Cooke's half-share of the London and Portsmouth Telegraph and the telegraph to Slough. Out of this amount Professor Wheat-

stone received 30,000*l.* in cash and 3,000*l.* for royalties then due, and Mr. Cooke received the remainder. I believe however that Mr. Cooke's personal share amounted practically to about 96,000*l.* of which the greater portion was in shares, many of which were subsequently disposed of at a loss.

During 1847 the Electric Telegraph Company erected their central station at the end of Founder's Court, Lothbury, the funds for this handsome building having been provided by Sir Samuel Morton Peto. It was formally opened on the 1st January 1848, and at this period 1,514 miles of telegraph were either erected or in progress.

The business was not an uninterrupted success. On the first day they took about 20*l.*, but this amount steadily increased each day.

The large hall was filled up with instruments from top to bottom, each gallery being appropriated to a different division of the country, and having a staff of clerks ready to receive and transmit messages. I believe at that time the cost of a twenty-word message from London to Glasgow or Edinburgh was about 17*s.* 6*d.*; to Yarmouth 9*s.* 6*d.*; to Ipswich 5*s.* 6*d.*; and to Southampton 3*s.*

It was soon found that they had over-rated the immediate capabilities of the telegraph traffic; they had spent all their capital, and their expenditure greatly exceeded their receipts. It became necessary to effect a great reduction in their expenses, and on the 27th March at one swoop they discharged about four-fifths of their clerks, who were however re-engaged as their prospects improved.

The year which followed was one of great commercial disaster consequent on the French Revolution and the abdication of Louis Philippe. By the month of June 1848 the operations of the Company had resulted in the loss of 3,220*l.* 8*s.*, and the whole undertaking might have collapsed had not Mr. Ricardo advanced money, and taken upon himself the burden of other shareholders whose confidence in the Company had ceased. They were at this time receiving about 100*l.* per week for messages, and by December their actual loss had been reduced to 341*l.* 0*s.* 11*d.*

In January 1849 they were able for the first time to speak

direct without delay from London to Birmingham and Manchester. This was considered a great telegraphic feat.

In the year 1850 their gross revenue from all sources was 43,524*l.* 3*s.* 9*d.* out of which they made a profit of 10,075*l.* 12*s.* 3*d.*

In 1851, the year of the Great Exhibition, their gross revenue was 49,866*l.* In 1852 their receipts for messages amounted to 100*l.* per day. In 1860 their revenue had increased to 214,245*l.* 7*s.* 3*d.* and their profits to 69,711*l.* 14*s.*

The 30th June 1868 was the day fixed for the transfer of the whole system to the Post Office Department of Her Majesty's Government, but owing to various delays the actual transfer did not take place till the 28th January, 1870.

Their receipts for the thirteen months ending on this date were 425,789*l.* 2*s.* and their profits 202,480*l.* 6*s.* 2*d.*

On finally winding-up the Company, in addition to interim dividends (which were limited by their Act to 10 per cent. per annum), the shareholders divided among themselves a sum of 2,938,826*l.* 9*s.* received from the Government, and a Trust Fund of 40,721*l.* 17*s.*, being equal to a dividend of 292*l.* 1*s.* 3*d.* per cent. upon their capital.

The Electric Telegraph Company was not however allowed to pursue its way without opposition. In July 1850 the British Electric Telegraph Company obtained their Act, their engineer being Mr. Edward Highton, and in the same year the Magnetic Telegraph Company was originated, their engineer being Mr. Charles Bright, and at first they employed the electro-magnetic instrument of Mr. Henley.

These two Companies afterwards amalgamated and became a powerful rival of the Electric Telegraph Company. The United Kingdom Telegraph Company obtained their Act in 1861, their engineer being Mr. Andrews, and they also erected an extensive system of telegraphs and introduced the Hughes Printing Telegraph.

The Electric Telegraph Company endeavoured to establish a practical monopoly by either opposing or purchasing the inventions of rival patentees. Among these the chemical printing tele-

graph of Mr. Alexander Bain deserves especial notice. Chemical telegraphs were suggested at an early date, and in 1838 Mr. Edward Davy patented a chemically-marking telegraph of considerable merit, employing calico tapes moistened with iodide of potassium. In December 1846 Mr. Bain patented his system, and in addition to the use of an iron style resting on paper moistened with a solution of ferro-cyanide of potassium described the important principle of setting up the messages on perforated paper, a system which has done more to increase the capabilities of the telegraph than any other invention.

That invention was exhibited to the Electric Telegraph Company, and while being examined one of the regulating springs broke and allowed the instrument to travel round with uncontrolled speed. To their surprise they found that the whole message was visible and had been transmitted correctly at the rate of several hundred words per minute, upon which they resolved to purchase the invention without delay. I believe Mr. Bain received 7,000*l.* for his patent and for the withdrawal of his opposition to their Bill. They employed the system of printing on chemical paper for some years, until it was eventually supplanted by the Morse inking system. Nothing however was done with the punched paper until Sir Charles Wheatstone introduced his very beautiful automatic printing telegraph, which is the most rapid system of telegraphing at present in ordinary use.

The real capabilities of the Bain system remain however to be yet developed. The Americans have recently re-introduced it with startling results, and have shown that on ordinary circuits four hundred or five hundred words per minute may be readily transmitted by its means.

When the capabilities of this system become generally known to the public, they will doubtless insist on enjoying the advantages to be derived from it, either in the form of lengthened messages or a lowered tariff. It appears to me, that in order to obtain the full benefits of telegraphic communication any reduction in the cost should be accompanied by the introduction of *express messages*, a species of message bearing the same relation to ordinary messages

that passenger trains bear to goods trains. The cost of these messages should be at least five or ten times as great as that of ordinary messages, and they should be subject to the same rules of priority among themselves as now exist, but they should in all cases take precedence of the ordinary heavy traffic. Without some such system much of the celerity to which we are now accustomed will be lost amidst the enormous accumulation of work which must sooner or later fall upon the telegraphic system of this country. The Electric Telegraph is quite capable of transmitting a large portion of the business of the country which is now transacted by letter, and is being so employed more and more every day. If this expansion of traffic be accompanied by facilities for securing rapid transmission for important messages the pecuniary gain to the "Post-office" will be very great, while the benefits afforded to the commerce of the country will be enormous.

I regret that I cannot now pursue this interesting subject further; and I must now take leave of the Electric Telegraph Company, with which I was so long and pleasantly associated, merely recording that its successive Engineers were Mr. W. H. Hatcher, Mr. Edwin Clark, Mr. Latimer Clark, Mr. Cromwell F. Varley, and Mr. Richard S. Culley. The Company has now become merged in the Telegraphic Department of Her Majesty's Post-office, and under the able administration of that department its growth and progress have outstripped the most sanguine calculations.

In 1865 the six existing Telegraph Companies possessed 16,066 miles of line and 77,440 miles of wire; the number of messages transmitted was 4,662,687, and 2,040 offices were open to the public.

On the 30th of last June, the Post-office system comprehended 106,730 miles of wire, and 1,451 miles of submarine wire, exclusive of Railway Companies' wires, and of the Continental and other cables of the various Telegraph Companies. The number of telegraph offices open to the public on the 31st of December was 5,572, and the number of telegraphic instruments in commercial

use was 9,220. The growth of the traffic may be seen from the number of messages, which was as follows:—

December,	1865	4,662,687
,,	1871	11,760,518
,,	1872	14,858,020
,,	1873	17,294,334
,,	1874	19,116,634

It is gratifying to observe that the consolidation of the telegraphs into a Government system has in no way tended to retard the progress of telegraphy, or to discourage invention. Although the able officers of the Government must have been at times hardly taxed to meet the growing requirements of the service and the difficulties incident to the transfer of a vast network of rival telegraphs and their consolidation in one centralised system, they have been throughout among the foremost to seize upon every scientific invention or idea, and to test its practical adaptability to the wants of their system.

I could have wished to have touched further upon the history of the other Telegraph Companies, to have spoken of the introduction of the Pneumatic system, of the various automatic and type-printing telegraphs, of the duplex system, and of the exquisitely scientific instruments of our retiring President Sir William Thomson; also of the history and development of submarine telegraphs, of underground wires, and of the progress of telegraphy in other countries: but time forbids, and I must reserve space for a few words about ourselves.

I believe the present Society owes its existence chiefly to the wisdom and energy of Major Frank Bolton and Major Webber, R.E., who foresaw the probable success of the institution, and the benefits which it would confer on all engaged in telegraphic practice or electrical research. As we are all aware, it has been warmly supported by the profession both at home and in foreign countries, and, what is still more gratifying, our list of members comprises the names of some of the most eminent scientific men of the age, and of many who are entirely unconnected with

telegraphy. I should like to see this division of our forces greatly extended.

I remark with pleasure that several of our great Submarine Telegraph Companies have given us cordial support, and their officers, who are so exceptionally well circumstanced for making observations of the highest value, have contributed admirable papers to our Transactions. We have also been fortunate in receiving the valuable co-operation of the department of Royal Engineers, and many of our best papers have emanated from that highly scientific body.

Our Journal continues to maintain its character. It has now reached its seventh number, and is becoming a work of considerable historic and scientific value.

Our example is beginning to be followed in other countries, and already an American Electrical Society has been constituted in Chicago.

The present number of our Members is 650.

I will not occupy your time by giving a list of the telegraphic works which have been executed during the past year, since they are well known to most of us, and are abundantly recorded elsewhere. But I will refer to a few of the more interesting novelties which have recently occupied our attention. Among these, I give the first place to the re-introduction of the Bain system of telegraphing by punched paper and chemical decomposition, to which I have alluded in the earlier part of my discourse—if the promises which this system appears to hold out are realized, it will have a powerful influence on telegraphy.

The next discovery I would notice is that of Mr. Edison, of Newark, U.S., who has made the interesting observation that when an electric current traverses a strip of paper moistened with certain solutions it acquires an extremely slippery surface, and taking advantage of this, he has constructed an instrument which may hereafter prove of much value in telegraphy.

Mr. Elisha Gray, of Chicago, has turned his attention to the transmission of signals by sound; he has exhibited instruments by which musical sounds and even chords are perfectly transmitted

over telegraphic wires, and by his latest researches he finds that seven or eight or more different sounds can be all transmitted simultaneously over one wire, and, by springs vibrating in unison with the several notes, can be separated at the end. He is now engaged in applying this principle to telegraphy with prospects of success.

M. Clamont of Paris has so improved the well-known thermo-electro pile as to render it probable that it may to a great extent supersede the use of the ordinary voltaic battery.

The Duplex system, now so well known, has been also re-introduced from America, and I allude to it here, not only to point out the activity of thought and invention that is now going on in America and on the Continent, but to show how much remains to be done by original research and experiment, and how desirable it is that we should re-investigate, with the aid of the improved knowledge and appliances we now possess, the inventions and ideas of our predecessors. A boundless field now lies open to research, and I trust that before long this Society may be in a position to offer the advantages of a laboratory, of electrical instruments, and of artificial lines and cables, to any of its members who may desire to prosecute fresh researches.

I have no doubt the most interesting and gratifying part of my address will be the announcement I am able to make to you this evening, that the acquisition by this Society of the valuable library of our late lamented member Sir Francis Ronalds is now complete. Our sorrow for his loss is tempered by the remembrance that he lived to witness, to an extent perhaps never before vouchsafed to man, the wondrous success and development of that telegraphic system which he had done so much to perfect and to advance in his early life, and by the gratification which we know it afforded him to receive at the hands of his Sovereign a well-deserved recognition of his services to his country.

By his will he bequeathed his library, which it had been the amusement of his life to perfect, to his brother-in-law, Mr. Samuel Carter, of Battle, and this gentleman, in fulfilment of Sir Francis Ronalds's desire that the library should be made available to all

students of electricity, has transferred the whole in trust to this Society, with a reversion to the Royal Society, of which he was so distinguished a fellow, in the event of this Society becoming extinct. The deeds are now before me, approved, and only requiring execution. Among other provisions, it is stipulated that the collection shall be termed the "Ronalds Library," and that we shall at once publish the complete catalogue, which it has been the labour of his life to perfect; also that under due restrictions the library shall, as far as possible, be open to all who desire to consult it, and that we should apply for a charter of incorporation.

I do not yet know the precise extent of the library, but I observe that in a letter of the 29th March, 1870, to the Right Hon. W. E. Gladstone, acknowledging his intimation that Her Majesty had expressed her intention of conferring the honour of knighthood on him, he says, "this procedure may tend, in some small measure, to promote my endeavour to complete a much-required Electrical Library, of which about 10,000 books and other writings are collected, the fruit of many years' search, and which I intend to bequeath or give to public use."*

This valuable collection of works will shortly be transferred to our rooms at Broad Sanctuary; and, supplemented by the complete copy of the Transactions of the Royal Society, recently presented to us by Mr. Louis Crossley, of Halifax, and by the gifts of others already promised or presented to us, and by our own purchases, it will form one of the most complete special libraries in the world.

I can assure our Members they will find the older writers, as well as the more modern ones, well worth their attentive perusal, and full of suggestive thought and experiment. Occasionally they will meet with surprises as regards priority of discovery. Even in my own cursory reading I have been interested to observe that Galvani was not the first to notice the galvanic convulsions of the frog;† that Volta was not the first to construct the Voltaic pile;‡

* It is believed that the Catalogue contains a list of about 10,000 works and pamphlets, and that the library consists of about 5,000.

† Hist. and Memoirs Royal Acad. Sciences at Paris, Martyn, 1742, p. 187.

‡ Encycl. Brit. 1860, vol. i. p. 963.

that that elegant instrument the Peltier electrometer was an English invention of the last century;* that Oersted was not the first to observe the influence of the galvanic current on the magnetised needle;† that Wheatstone was not the earliest originator of the electric balance;‡ and that Thomson was not the first to use the instrument we familiarly know as the "mouse mill," or to perform the beautiful experiment of dropping zinc filings through a copper funnel in order to discover the difference of potential induced by the two metals.§ Some of these cases illustrate a well-known law concerning the simultaneous introduction of new inventions.

There is only one other subject on which I will detain you this evening, and that subject is no other than the constitution of the Society itself.

This Society, as you are well aware, has been modelled on the lines of the parent Society—the Institution of Civil Engineers; our rules, our constitution, and our proceedings, have all been closely copied from theirs. Our object is the general advancement of electrical and telegraphic science, and most fully have the hopes and intentions of the founders of the Society been thus far realised.

As a natural result of our close imitation of the illustrious body in whose rooms we are now assembled, our proceedings and constitution have assumed, in a marked degree, a technical character, and there are those among us, and I confess I am one among the number, who consider that, while giving the highest consideration to practical telegraphy and applied electricity, we shall fail in covering all the ground which rightfully belongs to us if we do not equally cultivate both of the objects of the Society, and endeavour to attract the lovers of pure science, and to make ourselves as much an Electrical Society as a Society of Telegraph Engineers. Any

* Milner's Experiments on Electricity, 1783, p. 1.

† Journal Society of Arts, April 23, 1858. Hamel, Hist. of Teleg. 1859, p. 34.

‡ Journal Society of Teleg. Engineers, vol. i. p. 206.

§ Bennet's New Experiments in Electricity, 1789. Nicholson, Phil. Trans. 1788. Cuthbertson's Electricity, 1821, p. 390.

one who will revert to the inaugural address of our distinguished first President, and to the observations of those who spoke on that occasion, will perceive that that feeling is a very prevalent one, and one worthy of our attention. It is true that our technical character is, in one sense, a great source of strength, and it has, doubtless, been the means of attracting many Members, who join our ranks, partly on account of the professional value and interest of our papers, and partly from a feeling of professional *esprit de corps*. It would be most unwise to take any steps which could by possibility weaken this feeling, but at the same time it behoves us to observe that we have not yet enrolled in our ranks, to any great extent, that large body of private scientific workers who love and pursue the science of Electricity without any thought of regarding it as a profession. The earliest society of this character—the "London Electrical Society," which was established in 1841 under the able presidency of our distinguished Member Mr. C. V. Walker, F.R.S. and whose proceedings form a valuable contribution to the history of Electricity—relied entirely on the support of this class of members; and the rapidly increasing appreciation and love of physical science has caused, and will cause, their numbers to increase immensely; and it is among such as these that we may confidently look for the brightest discoveries of electric research. Now many of these will ask themselves, What right have I to consider myself eligible among a Society of Telegraph Engineers, or what affinity have I with them?

The Royal Society will ever attract to itself the most important papers on subjects of high philosophical research, but other societies will certainly arise to fill the electric void if we leave it vacant. Already the want has been felt, and a *Physical Society* has been constituted, which is destined, doubtless, to attract many lovers of pure electrical science, who would willingly join our ranks if the character of our Society were more adapted to their requirements.

It has been suggested that some friendly alliance or amalgamation might be formed wlth this young but important Society, and if anything of the kind is to be attempted, or any effort is to

be made to give our own Society a more purely scientific character, it is evident that it should be done soon or not at all. Possessed of these views, I have gladly welcomed a proposition that I should become a Member of the Council of the "Physical Society," and, should I be elected to that office, would use my best efforts for the harmony and welfare of both Societies. It is probable that this important question will be again brought under your notice, but in the meantime I have thought it of sufficient interest to address you upon it from the chair.

I have now, Gentlemen, to thank you for the patience with which you have listened to my remarks, and to ask you all to lend your earnest assistance and co-operation throughout the year in endeavouring to increase the influence and prosperity of the Society of Telegraph Engineers.

Mr. William Hooper: Gentlemen, I am sure I need not trespass upon your time at any length in asking you to join with me in a hearty vote of thanks to our President, Mr. Latimer Clark, for the very elaborate and interesting address which he has given us, and at the same time to request him to allow it to be printed. I have followed him, as I am sure you all must have done, with the greatest pleasure and interest, through the history he has given us of the electric telegraph from its earliest stages down to the present day, and I think it is due from us that on the present occasion we should record our sense of the great services which our excellent President has rendered personally in the practical advancement of the science of telegraphy and the adaptation and application of it to the great commercial and social requirements of the age in which we live. Scarcely any man has more contributed to that than our President, Mr. Latimer Clark. From my earliest days I took up this subject with very great interest, and I have followed it up, I may say, with some knowledge of chemistry, until I have witnessed the great success to which it has attained, and I repeat, in the consummation of that result, there are few persons to whom we are more

indebted than to our President. With reference to the mention that has been made of the library of the late Sir Francis Ronalds, I may state that through the courtesy of Mr. Samuel Carter I have had the privilege and pleasure of going through that library. It has been very properly described as one of the most valuable technical libraries in the world, and it must be a very gratifying fact to Mr. Latimer Clark that under his Presidency it has been consigned to the keeping of this Society. The younger members of the profession, as well as we who are older in it, will find many works to interest them, and from which they can obtain a great deal of information. The Society may be congratulated upon the circumstance that this library has been accorded to it as a free gift. The recognition of Sir Francis Ronalds great services in the cause of telegraphy came, as we all know, late in life. I believe he was approaching four score years and ten when the late Premier recommended Her Majesty to bestow upon him the honour of knighthood. We may, however, congratulate ourselves upon the interesting fact communicated to us this evening, that this unique collection of works on Electricity and Telegraphy has now absolutely come into the possession of this Society, and will be made available to all its members. In conclusion, I beg to call upon the members present to join me in a cordial vote of thanks to our President for the very excellent address he has favoured us with.

Professor Hughes: I beg very cordially to second the proposition of Mr. Hooper. I have listened with great pleasure to the President's admirable address, and some of the suggestions he has made I think are very important, more particularly that when we are rich enough we should establish a laboratory of our own, for there are many experiments which amateurs cannot carry out themselves, and it would be a great assistance if it should be possible to carry this out. I have great pleasure in seconding the vote of thanks.

The proposition was carried by acclamation, and, the President having announced that Mr. Samuel Carter had this evening been elected an Honorary Member of the Society, the meeting adjourned.

The Thirty-second Ordinary General Meeting was held on Wednesday, January 27th, 1875, Professor FOSTER, Vice-President, in the Chair.

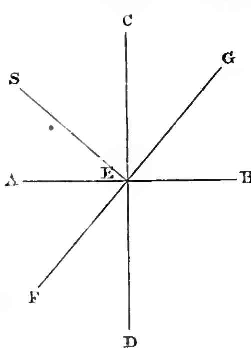
The first paper read was—

ON THE HELIOGRAPH OF MR. HENRY MANCE,

By Mr. CHARLES BECKER.

Before entering upon a description of the Heliograph it ought to be remarked that the name given to the instrument by Mr. Mance is apt to give rise to some confusion. The word Heliograph, in itself a good one, has been for years used to denominate at Kew Observatory and elsewhere an Equatorial adapted to take photographic pictures of the sun. The designation, "Sun Telegraph," which Mr. Mance also proposes, although not so euphonious, appears to be preferable, as it admits of no misconstruction.

To use flashes of sunlight reflected from mirrors as signals is by no means new. When a carefully-worked plano-parallel mirror of very small dimensions is used it is incredible to what distance the sunlight may be transmitted. As far back as the year 1821, Professor Gauss, of Göttingen, constructed his celebrated Heliotrop. The construction is very simple yet ingenious. If rays of light from



a very distant luminous body fall upon two mirrors which are at right angles to each other, the rays will be reflected from these mirrors in opposite directions. Let A B and C D be two mirrors at right angles to each other, then the ray S E will be reflected from the mirror A B in the direction E G, from the mirror C D in the direction E F, both being in the same line. Suppose such a system to be placed before the object-glass end of the telescope of a surveying instrument, say a theodolite

or a level, and one of the mirrors to be of black glass and very small, then the observer would know that the reflected image of the sun by the larger mirror would reach the distant observer when he sees the image of the sun in the small black mirror at the same time as he sees the locality of the distant observer through the telescope. By this method a mirror of only a few square inches surface has been found sufficient to transmit flashes of sunlight to great distances, and has been of great value in trigonometrical surveys.

The Trigonometrical Survey of India has used for many years heliotropes for similar purposes of simpler construction. Large mirrors of from 6 to 10 inches diameter, with horizontal and vertical motion, the mirrors of ordinary good plate-glass, were found insufficient; and we find it recorded that the surveyors years ago had arranged among themselves a more or less primitive mode of sun-telegraphy, by merely shutting off the light and so transmitting signals by interrupted flashes.

Through the general introduction of electric telegraphy, and the all but universal adoption of the Morse alphabet, it occurred to Mr. Mance, who has the advantage of a thorough knowledge of both, and the privilege, if it can be called one, of a long residence in a country with plenty of sunshine to develope and improve the existing appliances into a system. His aim has been to produce an instrument which is very compact, very portable, easily set up, and easily worked. Although he was first in favour of larger instruments (which are still preferable for permanent stations) he is now convinced that an instrument of the size here shown is all that is requisite.

The chief objection to the adoption of the sun-telegraphy is that we cannot command the sun to shine in the same manner as we have command over a galvanic battery, and it must be understood that Mr. Mance advocates his system only as an auxiliary to other systems of field-telegraphy; it would come into operation at distances when other methods are useless or tediously slow, and it compares favourably with existing systems of signalling in cheapness, range, rapidity of communication, and, lastly, in portability.

The flashes are invisible to any one placed far to the right or left of the direct line, so that from elevated points far distant communication could be kept up with a fortress without the besiegers having any suspicion of the fact.

It would be superfluous to give instances, when within the last few years the use of these instruments might have brought about the most important results.

The instrument consists of a light but firm tripod stand, similar to those used for prismatic compasses.

On the top a plate is moved by a tangent-screw which admits of quick and slow motion, and the plate carries on a pin a semicircular ring, which again carries on pivots the round mirror, the silvering of which is removed in the centre for a space of a circle about $\frac{3}{16}$ inches diameter. To the plate is also attached a simple key which is pressed down and springs back like an ordinary Morse key. This key is connected with the top rim of the mirror by a steel rod, which can be lengthened and shortened as occasion may require by turning the handle and screwing the rod through the small brass ball which secures it to the edge of the mirror.

By means of the last-named adjustment and the tangent-screw the glass can be altered as the overhanging position of the sun may require.

From 12 to 15 yards in front of the instrument is placed a sighting-rod. This rod is to mark a spot exactly in a line with the centre of the Heliograph and the distant station. A metal stud marks the spot, and a wooden cross-piece marks where the flash rests when not directed on the opposite station.

The instrument can be set up ready for working in a few minutes. When the exact position of the distant station is not known, a flash of sunlight must be thrown in the direction of the most likely points, and this must be continued till it is answered by a flash which indicates that a distant signalling-party is on the look-out.

Then after releasing the tangent-screw the glass must be turned to a convenient angle, and the sighting-stick must be directed in a line with the distant station by looking through the small aperture in the centre of the mirror. When this is effected the stud must

be raised or lowered till it is in the line of vision on a level with the centre of the glass, and the distant flash and the short cross-piece must be placed at right angles to the upright, about a foot below the stud. After thus adjusted, the instrument ought not to be moved.

The spot will be observed gradually to rise or fall according to the direction in which the sun is apparently moving. The handle of the key, or the tangent-screw, or both, as the case may be, must be turned slightly after every two or three words to ensure, as far as possible, that the centre of the spot shall be on the stud when the key is pressed down.

When the sun is rather low in the heavens and behind the signaller, it becomes more difficult to direct the flash with accuracy. In consequence of the obtuseness of the angle the spot loses its circular form, and becomes rather dim when reflected on the stick. If it is required to work frequently with the sun in this position, the employment of a second glass on a light tripod stand is recommended.

But it would be useless here to enter more into the minutiæ of working the instrument; suffice it to say, that in experienced hands twelve words per minute have been obtained, while others state that men, after a fortnight's practice, could attain only from four to five words per minute. As to the distance, ten and twenty miles, and, in very clear weather, forty miles, have been obtained.

A number of officers of the Indian army have tried and reported upon these instruments, and, with the exception of one, all report most favourably of the system as an auxiliary to existing systems, and efforts are being made at the present moment to ensure their adoption for the Indian service by the authorities.

At the Chairman's request Mr. Becker illustrated the working of the instruments exhibited in the room.

Mr. Goode, being requested by the author of the paper to give further explanation, said: The instrument consists of a mirror

placed on a tripod with due means of adjusting its inclination according to the position of the sun and the place to which signals are to be sent, and of a lever or telegraph-key to alter that inclination so as to throw the reflected flashes of light on and off a given spot. The adjustments are so contrived that they may be easily thrown out of gear to admit of the mirror being revolved freely by the hand to its approximately correct position, and then by restoring connection to move it with fineness and exactitude in accordance with the (apparent) motion of the sun. The free revolution of the mirror serves another purpose. When the position of the observer is distant and not accurately known, a flash of light can be thrown round the horizon right and left to attract attention, which, if a look-out is kept, it soon succeeds in doing.

The object of the sighting-stick is this. There is a little hole in the centre of the mirror through which the signaller looks towards the place he wishes to communicate with. At a short distance from the mirror the stick is placed exactly in a line between the centre of the mirror and the distant object. On the stick are two moveable metal nuts. The top one is moved up or down until it is in a true line between the mirror and the distant station. The lower nut, which carries a wooden rod about a foot long, is then placed a few inches beneath; the short rod being across and at right angles to the sighting-stick. It will be observed that the true line of communication is the upper stud. When the instrument is at rest the sun-flash thrown by the mirror does not rest upon the stud but on the cross stick beneath, but when the mirror is inclined a littlo by the pressure of the lever key it lifts the flash just up to the top stud. The signaller knows when he sees the light there that the flash is sent in a direct line to the person to whom he wishes to signal, and he has only to observe that the flash each time he presses the key rises to that stud to feel assured that his aim is true. These flashes can be seen in ordinary sunny weather at a distance of 50 miles in our own climate, and at 70 to 100 miles in a climate like that of India; in fact there is no limit to the distance except the obtrusion of the rotundity of the earth. It will

be observed that as the sun is constantly moving, the adjustment of the mirror to keep the flashes in a true direction must also be constant, and this can be done while the signalling is in progress. The Heliograph thus affords a ready and simple means of holding communications between any two stations, no matter how distant if nothing intervenes, and it is therefore a telegraphic apparatus complete in itself. But of course it is not of the same universal application as the electric telegraph, inasmuch as it can only be used in the day time when the sun is shining, and between positions where nothing intervenes. But there are many countries in the world where the sun shines in the daytime almost throughout the year —in India for example. Many such countries are but partially developed, where electric telegraphs would not pay, and it may be taken for granted they will not therefore be laid. It is believed that in such countries the Heliograph might be brought into ordinary use, and that it would prove a valuable auxiliary or substitute for the electric telegraph. But the chief thing Mr. Mance has had in mind in the construction of the Heliograph is to make it suitable for military purposes. With that view it has been constructed as light and portable and as little liable to injury from rough treatment as possible. The whole apparatus with the cases weighs only 6 lbs., and it can be easily carried by a soldier. You are aware that the means at present employed for communication between detached parties in the field is by flags, whilst in the extended operations of war the field telegraph is, where practicable, laid down along the main lines of communication; at shorter distances flags are waved in the air, and signals according to the Morse or other code of long and short motions are transmitted. It is impossible that the Heliograph should take the place of flags, because it cannot be relied on in all weathers, but the far wider range and greater speed attained by it suggest it as the natural auxiliary and complement of flags, the one or the other being used according to the surrounding conditions.

The shortcomings of the Heliograph in comparison with the electric telegraph are so obvious that it is worth while to mention one or two of its advantages, viz., cheapness of production, the

non-necessity of the laying or maintenance of wires, and the establishment of communication between positions where the laying or maintenance of wires would from physical or political causes be impossible. More than this, the telegraph can communicate only in the direction of the wires; the Heliograph can send messages in every direction with equal facility; and whereas wires may be cut or tapped, the Heliograph may be used with the most perfect secrecy and security.

The originality of principle in this instrument claimed by Mr. Mance is the adoption of a telegraph key to a mirror, by which he makes the mirror an active agent instead of a passive one, and so endows it with the capability of transmitting verbal messages, and of receiving them in return. The Heliograph is, it is believed, the first sun-reflecting instrument ever constructed avowedly as a talking machine, and differs as essentially from a heliostat or heliotrope as does our sun from one which should send his rays in intermittent pulsations with a desire to enter into conversation by means of the Morse code. As regards the distance at which the signals can be seen, a report from the department of the Quartermaster-General of India, embodying the results of experiments made by officers specially instructed to carry them out, says: "All the reports agree that the signals are perfectly clear and satisfactory; that they can be easily read in ordinary weather up to a distance of 50 miles." It may seem a little incredible that a sun-flash can be seen at so great a distance as 50 or 100 miles, but a flash from the Heliograph is like a flash from the sun itself, and the reflected rays make observers as sensible of their presence, at whatever distance, as though they came direct from the sun. With regard to the value of the Heliograph in military operations, the same report recites cases in which "three or four Heliographs would be worth their weight in gold." As a further instance it states that in the late Abyssinian campaign, "where for want of wire the electric telegraph stopped at Antalo, a few Heliographs would have extended the means of almost instantaneous communition up to the walls of Magdala. As it was, mounted orderlies had to be used to convey messages along this part of the line."

Major Webber, R.E.: I think there is one feature in connection with this apparatus which is novel—at least I think it is new—and that is that the signals are transmitted by the obliteration of the light, and not as is generally done with the instrument now in use, by the exposure of the light. Some time ago I heard of this apparatus being used during military manœuvres in the Madras Presidency, and an officer told me an instrument somewhat similar to this, but more simple in form, had been successfully used for signalling to distances of 20 or 25 miles. When he described this instrument it struck me there was a point which might be said to be a strong one in its favour: that is, by the mode of transmitting signals, as I have said, by the obliteration of constantly-exposed light, and not by the intermittent exposure of the light, as is usual with the lamp apparatus which is used for military and other purposes. In working signals sent by lamps at night it has often struck me that there was a source of delay which I did not see any way out of, but which this instrument I think has got rid of. When you are reading signals (of course there is a difference between day and night) the use of a bright light like this in daytime and the use of a lamp at night assumes the appearance of a small speck of light, there not being much difference in practice between the two. But when you are working at night with lamps through distance into space, you are obliged in the ordinary mode of signalling to watch for your light, and if your eye is not caught—that is, if you do not look actually in the right direction till there has been a succession of flashes which draw the eye—you may not see the first signal, or you may move your eye and lose the signal, and a repetition is required. With this system, with continuous exposure of light, you have something always to look at, and the signal is made by the obliteration during a short or long space of time to represent a dot or dash. I see my way to believing this system enables you to send signals at a much quicker rate than you can do by the old plan.

Colonel Stotherd, R.E.: The system of signalling, by the obliteration of the light, has been adopted for military purposes for the reasons given by Major Webber. In signalling by lamps at night we now use a steady continuous light to indicate the

position of a station and the obscuration of the flash for long and short intervals on the Morse principle to send a message. I may mention, with reference to Mr. Mance's instrument, that it has been tried at Chatham on two occasions. Instruments very similar to the form now exhibited were tried, and though they may not be applicable for signalling purposes in a country like this, where the sun only shines at intervals, still we saw enough to prove that, in a country like India, where sunshine is the rule, this system of signalling would be effective and practically useful, and I believe the reports upon it have been very favourable. There is a collateral point with reference to reflected sun flashes which may be interesting, namely, that it has been used for many years in carrying on the great triangulation of the Ordnance Survey of the United Kingdom, and observations for trigonometrical purposes have been taken by an instrument called the heliostat at distances up to 110 miles in connecting England, Scotland, and Ireland, for this great national work. It was found superior to any other apparatus. We tried the lime-light, arranged by the late Captain Drummond, R.E., for this purpose, but found the heliostat to be superior. It has been employed up to a distance of 110 miles, and could be seen even further. The heliostat is, in principle, precisely similar to Mr. Mance's apparatus, the reflected rays of the sun being used to produce the pencil of light.

Major BATEMAN-CHAMPAIN, R.E.: We cannot deny Mr. Mance the credit of having most ingeniously adapted two pretty well-known ideas. The heliotrope has been used as we know, for giving signals indicating locality, but not for giving news. Mr. Mance has combined the Morse system of signals with the use of the reflecting mirror, and although Major Webber seemed to think the key arrangement a little complex, I am inclined to say that you could hardly have a more simple and handy instrument than this. I see no reason why an ordinary signaller accustomed to the Morse system should not work the instrument nearly as fast as the ordinary key instrument. The signals can be sent by obliterating the light, or they may be sent, if preferred, by throwing the light in long and short flashes. The

apparatus appears to me light, handy, and easy to adjust. The arrangement of the cross-stick is simple, and I know from the trials made that the rays of light can be directed with great accuracy upon distant objects. I have always thought it would have been a very valuable instrument on occasions, such for example as the siege of Paris, where it might in some ways have given results not otherwise attainable. For instance, in time of sunshine signals could have been sent to a single concealed individual 20 or 30 miles distant, while it would have been impossible for the enemy round the city to know that the signals were passing. Nobody could see the flashes except the people to whom they were directed. I think this is a valuable adjunct to the other more regular methods of communication, especially in countries like India, where for the greater part of the year the sun's rays can be surely counted on.

The CHAIRMAN: If no other gentleman has any further observations to offer, I will venture to make one remark, although I have no practical experience in the use of instruments of this kind. The merits of instruments such as this, which do not involve anything new in principle, depends on the way in which the details of their construction are adapted to the purpose for which they are intended, and it appears to me that there is one contrivance connected with the instrument before us to which previous speakers have not specially alluded, which is worth pointing out, on account of its simplicity and obvious efficiency. I refer to the way in which the light is kept directed upon the station with which communication is to be made. This, as I understand, is effected by removing the silvering from a small part of the centre of the mirror, so as to obtain a transparent spot from which there is little reflection. A fixed line is thus obtained, determined by the centre of the mirror and the centre of the stud, and at the same time the operator can tell, by looking through the unsilvered part of the mirror at the stud, and keeping the reflection from the middle of the mirror upon it, that the light is being thrown in the required direction.

The second Paper read was :—

ON VIBRATIONS DUE TO EARTH-PLATES.

By James Graves.

Although six years have elapsed since the transfer of the working apparatus from the Valentia temporary station to the new buildings, it may not be uninteresting to the members of the Society if I recount the difficulties which I met with in establishing the working from the new station upon a satisfactory basis, as there is a something in connection with this subject which requires elucidation, and which may have been experienced by others elsewhere, and, if so, members may now have an opportunity of bringing their experience forward for the benefit of the Society, and in advancement of the science with which it is so intimately connected.

At this station the battery room, cable instrument room, and land instrument room, join end to end, and form part of one side of the building on the basement floor.

In fitting up the offices I led all the battery wires from the battery room to the instrument rooms through the walls and under the floors to their respective instruments.

I provided five separate earth-wires, numbered 1 to 5 in the plan. Nos. 1 and 2 were thick copper wires, buried naked in trenches for about thirty yards, in opposite directions, and terminating in pits, into which fresh water drained. No. 3 was the iron sheathing of a cable laid to low-water mark in the beach, and about 200 yards long. Northward from the station, No. 4 was a copper plate, about a yard square, soldered to the stranded conductor of this cable, and buried in the beach. No. 5 was another copper plate, on the end of the conductor of a similar cable, laid to the southward from the station; Nos. 4 and 5 being about 300 yards apart.

Nos. 1 and 2 were intended for working the 1865 and 1866 cables respectively. Nos. 3 and 4, iron and copper, were intended for testing purposes, and No. 5 for working the land-lines to London.

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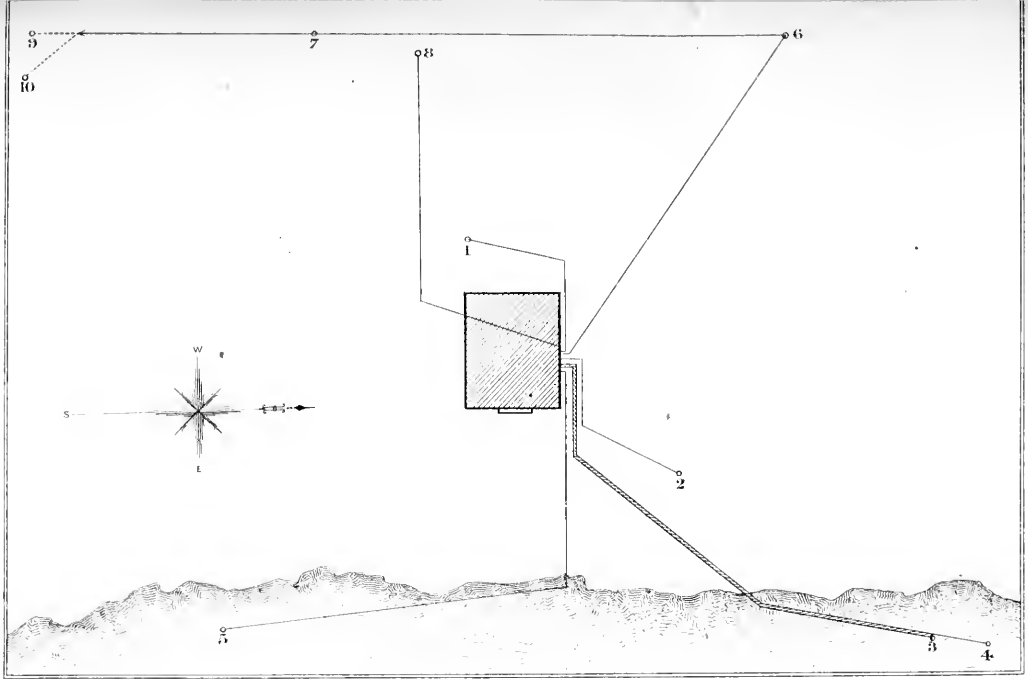

PLAN OF EARTH-WIRES.

Nº 1. *Buried thick copper wire.*	Nº 6. *Aerial wire on poles 1/8 mile distant.*
„ 2. *do do*	„ 7. *do 1 mile do*
„ 3. *Iron of cable buried in beach N. of Station.*	„ 8. *Iron pipes to Water cistern in field.*
„ 4. *Copper plate on Conductor of Cable buried to low water mark North of Station.*	„ 9. *Aerial wire on poles 5 miles distant.*
„ 5. *Copper plate on Conductor of Cable buried to low water mark South of Station.*	„ 10. *do do joined to shore end of Ocean cable.*

J. Graves.

Thus, I fondly imagined that as far as earth-wires were concerned, my arrangements were as complete as possible.

Two wires had been laid in iron pipes, between the old and new stations, in a trench, for five miles, intended for line-leading wires for the two cables, but it required little knowledge of the effects of induction to foresee that this arrangement would not work, for two gutta-percha covered wires laid side by side in the same iron pipes for such a distance were calculated to call forth rather powerful inductive phenomena, especially upon cable apparatus. It was not at all surprising, therefore, to find that to work two cables at the same time under such conditions was an utter impossibility.

However, as one of the cables was broken a month or two prior to the transfer from the old to the new station, it was not immediately necessary to work the two cables simultaneously.

Immediately after joining up the working apparatus I observed that there was a permanent vibration of the mirror and "spot," whether the cable or land-lines were working or not, but when either one or both of the land-wires were at work, particularly when sending, much more violent oscillations were noticed, which so confused the signals that the reading was most painful and uncertain.

By carefully observing the nature of the more violent disturbance I concluded it to be due to induction between the battery wires under the floors—those for land-lines passing in close proximity to those for the cable instrument. I therefore removed the battery for the cable from the battery room to the cable room and used fresh battery wires by a different route, and thus remedied that portion of the disturbance which was due to induction, but still there existed that unceasing trembling of the spot. It was not due to the vibration of the building, nor of the pedestal which was built in the ground, and quite clear from the floor; nor was it the result of the vibration of the earth, for if it had been any of these the effect would have been the same whether the galvanometer was short-circuited or not, whereas if short-circuited these vibrations ceased immediately; it was therefore an electrical effect from somewhere, and no conceivable permutations of the five

different earth-plates tended to reduce it in the slightest degree. "Vibrations" was the complaint of the staff from morning to night, day after day, and no remedy seemed available. "Vibrations" worried me by day and haunted me by night. Having but one cable for all the traffic it made experiments most tedious.

I may mention that I tried resistances between cable and apparatus—between apparatus and earth. I tried condensers in various positions, with and without branch leakages to earth. I used direct wires from batteries, and through the window to the line wire on poles to avoid possibility of induction. I insulated all the batteries most carefully and suspended the cable batteries by cable core, yet none of these things reduced the vibrations. The effect of inserting resistance in any part of the circuit was merely to reduce the amplitude of the signals, and of the vibrations in the same proportion. Reducing the sensitiveness of the galvanometer, or varying the battery power, had the like effect.

Although the earth wires, Nos. 3, 4, and 5, were buried under sea-water in immediate connection with the ocean, they gave no better results than those plates which were buried in the land; and believing that the disturbance arose from the earth, and finding no local earth-plate of any avail, I determined to try further off.

There were five miles of wire on poles between the old and new stations standing idle. I made earth by one of them one-eighth of a mile along the route (No. 6) by means of an iron stay, but that was no use. I then tried one mile off, but vibrations were the same with that earth (No. 7). I then connected up a wire for earth to the system of water-pipes leading 300 or 400 yards through the fields to the water cistern (No. 8) but found no advantage in that. I then went to the last pole but one of the five mile length and made earth (No. 9) by the iron stay, being within 50 yards of the buried shore end of the ocean cable; vibrations then disappeared and the spot was perfectly steady. Earth was thus, doubtless, virtually made by means of the shore end of the cable.

But it was most unsafe and unwise to have five miles of aërial wire connected with the apparatus for working an ocean cable,

therefore the next step I took was to join up the second wire in the iron pipes to the iron sheathing of the shore end of the cable and use it as an earth for working the 1865 cable. On making the corresponding alteration in the new station, this arrangement was found to be absolutely free from disturbance, and "vibrations" became for ever a thing of the past, to the great relief and comfort of all concerned. The two wires in the same pipes being used, one for line and the other for earth, gave perfect signals without the slightest vibration.

It thus became a recognised necessity to lay a pair of land cables for each ocean-cable between the landing-place and the working station. There are at present three pairs of these cables in one trench, each pair preserving the same relative position throughout the whole length of the trench; one pair of cables lying on each side of the central iron pipes containing the third pair, and a fourth pair lying in another trench on the opposite side of the road, and up to the present time no sign of inductive interference has been observed, but it is necessary to confine each pair of cables to their respective ocean-cable, as any crossing of them would produce inductive interference.

I have just watched three cables being worked to and from Heart's Content simultaneously without a shadow of interference one with the other, or the slightest sign of our old vibrations, although the shore ends of these cables lie close together for a considerable distance between high-water mark and the cable house, where all land and sea-cables concentrate into a test-box. A brief description of this test-box may here be given. Upon a wooden base four of Elliott's dry-air chambers are fixed. These consist each of an oak base hollowed in the interior, forming a shallow trough, in which is placed an ebonite bottom, supporting three ebonite pillars, with binding screws at the top, and also a strip of brass from the bottom of each, furnished with a binding screw; to the latter are fixed the sea-cable and land-cable, to the two outer screws, and the trough is then filled up with melted paraffin wax; the three pillars are covered with a brass cap with glass top, enclosing also a small glass containing pure sulphuric acid, to keep

the air dry; a cross connection is made inside the cap joining the land and sea-cables.

I have made some additions to these dry-air chambers. At the end of the trough, behind the brass cap, I have fixed two binding screws, or terminals, to one of which is connected the land-cable used for earth, and to the other the wire which is soldered to the iron of each shore end, the two terminals being joined across by a brass strap, which enables me to disconnect with facility the land-cable from its earth connection for testing its electrical perfection from time to time.

From the terminal in connection with the shore-end iron a wire is joined to the central pillar inside the dry chamber, making an earth connection between, but insulated from, the land and sea line-cables. A washer, into which is fixed a platinum point, is put under the nut of the sea and land cable binding-screws, pointing to the centre one. The latter has a washer with two platinum points, which, when in position, point to the two others, and thus form a lightning protector for each of the cables.

These four dry-air chambers are enclosed in a mahogany case, with a plate-glass top, which forms a most convenient and effectual test-box.

By means of the arrangements herein described, it is now possible to work four cables quite independently or simultaneously without the slightest interference with the signals, but the old vibration phenomenon can be reproduced at a moment's notice by simply joining up a local earth to any of the cables for working, proving that it was no temporary cause which affected the apparatus, prior to the remedy being discovered and applied, but a permanent influence, which can be brought to view at any time.

Such are the observed facts and the means used to remedy the evil; but what is the *cause* of these vibrations?

To trace this I think we must glance superficially at the geology of the island. On the west, north, and east the whole coast is rock, more or less raised above the sea level; this rock is chiefly of a slaty nature, and of variable quality; in some parts the cleavage is nearly horizontal, and in others at various angles, whilst on the

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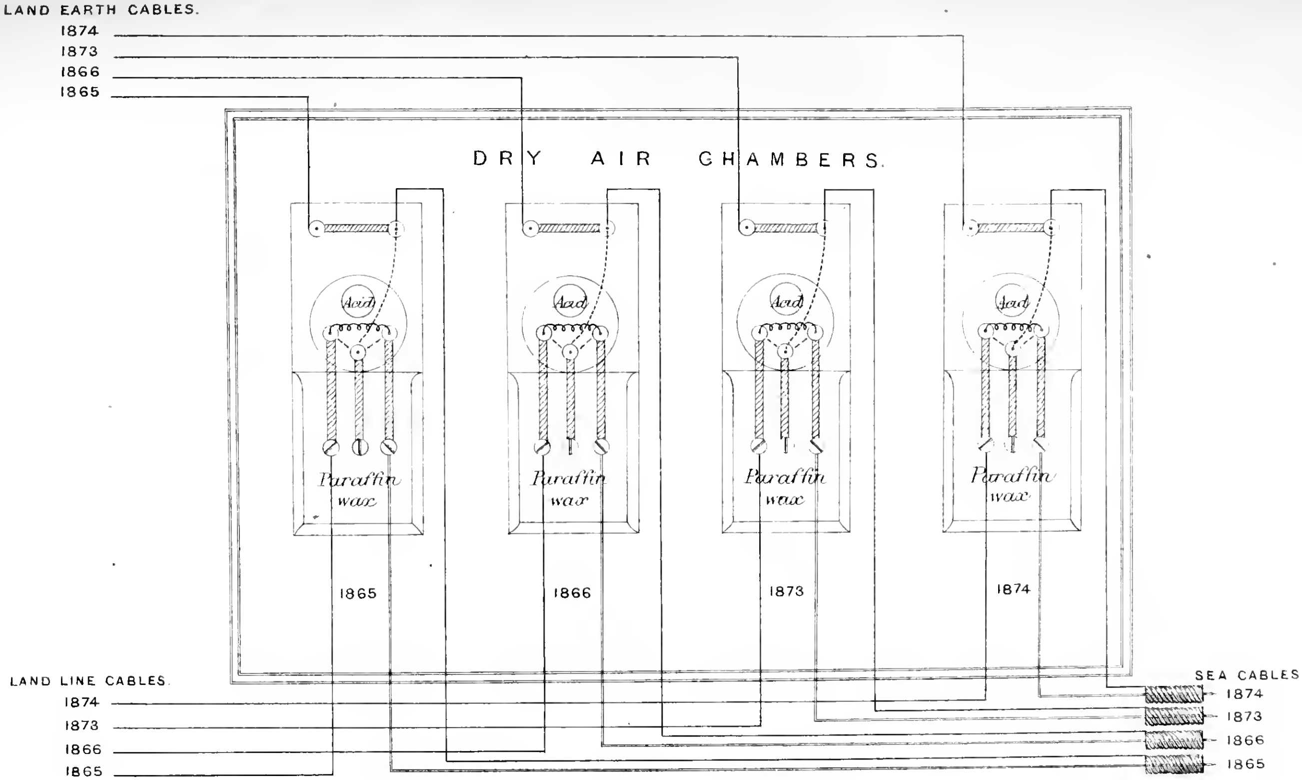


PLAN OF TEST-BOX.

AT FOILHOMMERRUM, VALENTIA.

south side of the island, which is but a few feet above high-water level, the cleavage of the slate-beds is nearly perpendicular. The hills are chiefly of slate; the celebrated Valentia Slate Quarry being in the highest one, rising 877 feet above sea-level. The island appears, therefore, to have a slate-bed, over which lies in the low ground a deposit of hard clay, containing large boulders and many large white quartz-stones, whilst in the valleys and flat ground are large fields of peat bog overlying the clay.

The new telegraph station, at the north-eastern end of the island, close by the shore, stands upon a bed of peat contained in a basin of clay, in a plot of land, with a frontage of 140 yards. The clay at each end is but a few inches from the surface, whilst in the centre it is 5 or 6 feet below the ground level, and this depth had to be cut through to reach the hard bottom on which to lay the foundation.

It will therefore be seen that any number of earth-plates buried in this basinful of spongy wet peat would virtually form but one plate, inasmuch as the dry hard clay, underlying the peat, and resting upon a rocky bottom, would prevent electrical currents from perfectly penetrating the earth's crust and so give a "bad earth," besides tending to promote polarisation between them, through the spongy medium, and this may be a probable cause of the difficulties experienced.

The earth-plates tested for resistance give an average of 5·16 ohms, but these resistances can scarcely be accepted as being the resistance offered between them and the earth's crust respectively, but rather as the resistance due to the plates and the medium between them.

But taking a land cable of a known resistance of 42·6 ohms, joining the farther end five miles off to the iron of the shore end of a cable, and testing its resistance with various earths at the station, I found No. 3, iron earth, gave an excess of 16 ohms, or say 8 ohms for each, while three different copper earths (Nos. 2, 4, & 5), gave 29·8 for their share, and a fourth copper (No. 1) gave 50 ohms; this was in higher ground and not so good as the others. Part of this 29·8 was due to battery current between iron and copper plates. But as the two iron earths gave only 8 ohms each, it does

not appear that the resistance to earth was very great: however, it is possible that, although five miles apart, the circuit between the plates may have been completed above the slate bed, and not have entered the earth (technically so called) at all.

Assuming that the plates gave "bad earth" only, one would suppose that interference with signals by vibrations would only occur when one or more of the lines were at work, and that, by finding a bad exit to earth, a portion of the working currents would charge the condensers of the cable apparatus through the medium of the earth-plate and so cause a vibration of the mirror. Were this the case it would not be so difficult to account for the phenomenon, but these vibrations are perpetual, never ceasing night or day from one year's end to another so long as the cable apparatus is connected to a local earth-plate, it being quite immaterial whether any other line is at work or not, and therefore they must arise from the earth itself.

Electricity is, I believe, credited with a large share in the formation of slate and other minerals, and in this island slate may be found in various stages of perfection. Can these vibrations be due to a perpetual series of charges and discharges in the slaty formations, the cleavage being nearly vertical in the vicinity of the station? Or what can the Members of the Society of Telegraph Engineers suggest for tracing satisfactorily the origin of these "vibrations?"

In the discussion upon "Earth Currents," on 12th March, 1873, this phenomenon was briefly referred to by Mr. C. F. Varley, and he remarked that "great discussion had taken place about the origin of these vibrations, which he had observed in 1858;" and Mr. Winter also stated that he had noticed the same thing, and had remedied its evil effects by the use of an induction coil, and hoped before his return to India to give further information on the subject, which would be very acceptable; but it would appear that, although we have discovered this phenomenon in different places, and applied different remedies, we have arrived no nearer to a satisfactory solution of its origin than we were sixteen years ago.

If any member can suggest anything with a view to tracing its

origin, and the manner in which these vibrations are produced, I shall be glad to carry out such suggestions as far as lies in my power.

Mr. W. H. Preece: I may add that the Paper has been perused by our President, and he has called attention to the fact that some years ago a somewhat similar phenomenon was observed in a part of Derbyshire, where great difficulty was experienced in establishing earth, and the wire had to be carried a considerable distance to make it. I may state further, that Professor Fleeming Jenkin, in *Nature*, describes an insulated island, St. Pierre, where the same phenomenon was experienced. A similar instance occurred to Mr. Culley in some experiments he made at Torquay, where it was found impossible to establish good earth till the wire was carried some distance into the sea. I remember myself a case in the chalk district, near Salisbury, where it was impossible to establish earth near the station, and the wire had to be carried two or three miles off, where a well was found which went deep into the chalk, so that the difficulties attending this matter, as well as the peculiar phenomenon mentioned by Mr. Graves, are well worth our study.

Mr. Morgan: I have made experiments with land lines from London. The idea was suggested to me by having to take observations for earth currents two years ago for Mr. Culley; the result was so interesting that I have frequently observed them since. I joined a galvanometer used for bridge testing, the resistance of which equalled 1,600 ohms, to wires leaving London in different directions, and invariably got a deflection of about 10° or 15° and sometimes up to 50°, but was unable to ascertain the reason for that result.

Lately I have been trying wires to the west of England going to Exeter, and on one of these I got a deflection of 50°, showing a positive current. It gradually went down to zero in 15 mins., and for an hour and 20 minutes it shewed a steady negative current ranging from 0° to 55°. I had the wire disconnected at Bristol,

and the deflection decreased from 55° negative to 15° negative, showing a sudden fall of 40°; the disconnection was distinctly seen, as it was done by time. After standing steadily at that for 15 minutes, I had it joined up at Bristol and disconnected at Exeter; the deflection rose to 35° and for about an hour the variations were very steady, ranging from 35° down to zero and up to 15° on the positive side; the fluctuation continued up to the time I had the wire joined up straight, showing about 3 hours and 20 minutes earth current reading from one wire.

I have tried several wires at different times, but not with such interesting results. I have also tried wires disconnected at Birmingham and Manchester, but the disconnection does not appear to alter the deflection much, that is to say, supposing a wire to give 20° of current, when the same wire is disconnected at the above-named stations the deflection does not fall more than 5°.

The approximate strength of these currents would be as follows. Supposing a circuit be formed of resistance coils ranging from 100 up to 20,000 ohms, one Daniell cell and the galvanometer used for the above experiments, let the resistance coils be plugged up, that is to say, with no resistance in the circuit excepting the one cell and galvanometer, and the deflection noted: it equalled in this case about 70°; now by introducing resistance until a deflection is obtained equal to the highest reading of earth-current—this would equal about —— ohms; of course, by increasing the resistance, the deflection will decrease, and if this be noted, say at every 5° of fall, a table can be made, giving the approximate values of the earth-current deflections, but, as we do not know the strength of the earth-current from where it enters the wire, accurate results cannot be obtained.

I have made a diagram, showing the variations of these currents; it is on the same principle of construction as a barometer chart, and shows the travel of the needle for 3 hours and 20 minutes.

Whenever I have had wires disconnected at a distance of 200 miles from London the fall of deflection is very small, and I can only suppose it to be due to earth-currents, but from what cause I am unable to say.

Current from one Daniell cell through galvanometer of 1,600 ohms = 67°:

Through 1,600 ohms. . .	+	480 ohms	= 60°
" " " . . .	"	960 "	= 55°
" " " . . .	"	1,560 "	= 50°
" " " . . .	"	2,200 "	= 45°
" " " . . .	"	3,400 "	= 40°
" " " . . .	"	3,840 "	= 35°
" " " . . .	"	5,000 "	= 30°
" " " . . .	"	6,400 "	= 25°
" " " . . .	"	8,400 "	= 20°
" " " . . .	"	9,400 "	= 15°
" " " . . .	"	15,400 "	= 10°

Mr. R. Gray: In the reading of the paper I did not catch whether Mr. Graves, in taking the cable wire off, tried to make earth at sea.

Mr. W. H. Preece: After he made earth from the external wires and cable, he continued to make use of that earth and no other.

Mr. Gray: Are you able to state whether the outside sheathing of the cable had at the same time earth on?

Mr. W. H. Preece: No; he had only one, and then the vibration stopped.

Mr. Gray: The question is, whether between the other end of the cable there was not some bad connection owing to some intermittent connection with the earth-plate, which would give a slight impulse now and then, and owing to the length of the cable it was made more perceptible; or perhaps there was bad connection with the earth itself.

Mr. Bordeaux: I recollect Mr. Varley telling me some years ago, in connection with a cable which was then being laid in the English Channel to connect the French Atlantic line with England, the earth used by the French Government Office being very close to the earth used in the French Atlantic Office, that the difficulty of working, in consequence of *variable* currents showing on the mirror instruments, was so great, that Mr. Varley laid a wire

direct to where the cable was landed at Minon, and formed a connection with the outside sheathing; therefore, I think the novelty of carrying the wire from the office to the cable is not altogether due to Mr. Graves. There can be no doubt the variable currents arose from the fact of the signals being received at the French Government Office communicating through the earth to the mirror instrument in the French Atlantic Office. I think Mr. Graves is right respecting the earth-plates, being so close, giving off these currents, but there is something curious in Mr. Graves' observations, showing the vibrations to be so continuous and so regular. It would be well if Mr. Graves could give us some further information as to whether the vibrations were measured, whether they were quite constant, or whether they were erratic, as that information might enable us to make some further researches into the matter.

Mr. Morrison: I remember when I was in Newfoundland, in 1858, we were there troubled very much with vibrations of this description. They were not always long ones such as were mentioned by a gentleman just now, but they were short. They looked very much like signals which might be received through a cable, but at that time we looked upon four words per minute as something very good; but there were also besides these short vibrations long vibrations, so that the spot of light moved backwards and forwards—moved altogether on the scale, so that the zero altered considerably. But curiously enough the station at Newfoundland was not where it is now but in the Bay of Bulls, and the ground was something like that described at Valentia. There was peat beneath the clay and I know there was difficulty in getting earth. We did all we could, and at the end we had not good earth; but the cable itself was so bad that we did not really know what was wrong: at any rate nothing was right and therefore we presumed everything was wrong. But still there was the fact that we had these vibrations and the same kind of earth described by Mr. Graves. The vibrations at Valentia, it seems, were got rid of by connecting up with the outside of the cable. One can understand in the case of two wires, one to earth, and one a cable wire, for five miles there would be no difficulty of

induction between these wires themselves or any other wires alongside, because those would be in equilibrium. These vibrations were however got rid of. It seems to me possible that something like this might occur: take the case of Valentia, earth at the American end of the cable might become changed from some cause or other not mentioned now to a different degree from earth at the eastern end of the cable. Now if the earth-plate at Valentia did not make good connection with earth, the best way for the current coming to that earth-plate might be through the core of the cable. If on the other hand the earth-plate at Valentia had good connection with earth, the quickest way for the current coming would be through the sheath. If that is the case where you have bad earth you must expect vibrations, and where you have got good earth you may be quite sure you will have none. I think the fact that the current may possibly in the one case come through the cable more easily than through the sheath may account for the vibrations; and I can understand in this particular case at Valentia the earth was about as bad as could be.

Mr. Donovan: I do not think the earth at Valentia was so bad as that, for ordinary telegraph purposes, the vibrations would have been noticed, but the instrument used was so sensitive that the slightest vibration gave a trembling on the galvanometer, which was a mirror galvanometer. I think in an ordinary land-line the earth-current would not have been noticed at all. I had thought it might be due to some chemical action on the earth-plate which affected the galvanometer.

The Chairman: I would ask Mr. Preece one question; it may be answered in the Paper, but I do not recollect if it is, viz., whether there is any special virtue in using the sheath of a particular cable at the earth-plate for that cable, which I understand Mr. Graves has done, or whether the effect of using the sheath of the cable is not simply equivalent to sending the earth-plate a long way out to sea? It seems to me that these rapid vibrations must have been due not simply to bad earth in the ordinary sense, nor again to earth-currents, but to a rapidly-varying electrification of the whole island, from whatever cause it may have arisen. If

this were the case we might expect that the effect would be got rid of by making communication with the ocean sufficiently far from the shore.

Mr. W. H. Preece: I gather from the paper, which I confess I only read when I read it here, that Mr. Graves made use of the outside wires of each cable, simply to avoid the possibility of induction occurring between the different wires, if they crossed each other. I gather also that the cables do not start from the same spot, and if the outside wires of each cable were not used for its own earth, they would cross each other, and by crossing obtain induction. The paper does not detail any experiments in that direction. It merely gives the fact, that, by making use of the external wires of each cable, Mr. Graves succeeded entirely in stopping these vibrations. I will say just one or two words. The subject of bad earths is one very well known to all telegraphists, and is one of the great difficulties experienced in establishing a new station. The fact of obtaining bad earth in the way Mr. Graves has explained shows that the earth practically forms part of every circuit, and it in no way carries out the old notion that the earth acts as a great reservoir for electricity. In the case of Valentia it is really what is called an insulated island, separated from the rest of the crust of the earth by some geological beds, which give a certain amount of resistance. The peculiar phenomenon observed consisted of vibrations, or a tremulous motion of the spot of light thrown by the galvanometer upon the reading scale when bad earth was used. Mr. Graves asks what is the cause of this? I apprehend it can be due to only two causes; first, to a variation in the potential of the earth; and, secondly, to a variation in the resistance of the circuit. We know that the potential of the earth is constantly varying, and we have evidence that earth-currents are always present; and those referred to by Mr. Morgan, between London and Exeter, are earth-currents proper, due to a variation of the potential of the earth. But in this case we have rapid changes, and it could not be due to any rapid change in the potential of the earth, because we should see it better when good earth is used than with bad earth. It must therefore be due to some variable resistance of the circuit due to

the imperfection of the earth-plate. I think it is due to electrolysis. The earth-currents themselves by their electrolytic action produce currents of polarisation on the earth-plates, which vary in the resistance of the earth-plate. The formation and removal of gas-bubbles cause these vibrations. We know that currents of polarisation are dependent upon or vary inversely on the surface exposed to electrolytic action. In the case of small exposed ends of broken cables, where the surface exposed is very small, these currents are strong, and the variation produced by them great. As we increase the size of the exposed surfaces, the amount of variation diminishes, until we reach earth-plates of infinite dimensions, like the external wires of a long cable when they disappear entirely. Hence, when the cable is idle these vibrations are simply due to the electrolytic action of earth-currents on the imperfect earth-plates.

APPENDIX.

In continuation of my paper upon "Vibrations due to Earth-plates," I append some extracts from the Station Diary, from the time of the first appearance of these vibrations to the time when the permanent remedy was applied, *i.e.*, from August 7th to November 14th, 1868; and on reviewing these "records of the past" it is difficult to come to any other conclusion than that arrived at, that these disturbances arose from the electro-polarization of the earth-plates, from the working batteries when in use, and when they were idle from the earth-currents in the cable, which in a former paper on "Earth-currents" I have shown are incessant, although of constantly varying force.

Extracts from Diary.

1868.

Aug. 7, from 6 p.m. while land-line at work, vibrations very strong; read NF (Newfoundland) with difficulty; tried another earth, but no improvement.

9·15 p.m. Great disturbance of spot when land-line working; impossible to work cable and land-line together.

Aug. 8, 9·20 p.m. Vibrations on mirror same as yesterday, gave land-line a fresh temporary earth; reduced vibrations a little, reading better.

Aug. 12, 10·15 p.m. Vibrations of spot during the day rendered reception from NF very difficult.

Aug. 13, 9 a.m. During the night used '66 (broken) cable as earth.

12·35 p.m. with usual earth vibrations very strong and troublesome.

5·20 p.m. Requested NF to use '66 cable as earth, VA (Valentia) doing the same; vibrations much reduced and reading with ease.

This arrangement appears to have been continued, as no further entries of vibrations occur until the old earth was tried again, on night of 18 Aug.

Aug. 19, 10 a.m. During night vibrations very strong, reading NF with difficulty.

6 p.m. Vibrations very troublesome all day while land-line working; requested NF to use '66 cable again for earth, and made same change at VA; vibrations reduced to a minimum, and able to read NF with ease.

During this day it was discovered that our two-mile earth-cable, which had been laid out to sea with a large mass of zinc on its sea-end, was broken on the beach, the end being washed-up above low-water mark. This earth-cable had been used for two years and upwards for working the land-wire, and thus carrying the powerful currents from the land apparatus well out to sea. There is, therefore, no doubt that this cable was broken on the 7th August, prior to 6 p.m., when "vibrations" were observed for the first time on the cable instruments.

Aug. 20, 9 p.m. Vibrations strong since noon, reading with difficulty and risk.

Aug. 22. Vibrations still continue, but not strong enough to materially affect reading. Occasionally, however, the spot is jerked off the scale.

From Aug. 22 to Sept. 7 the '66 cable appears to have been used for earth at both ends. No entries of vibrations during this period.

Sept. 27, 1·20 a.m. Vibrations and kicks very bad up to this time, now get very steady, and distinct reversals, spot moving half-an-inch each side of zero. Earth-currents very strong since midnight, holding needle of vertical tell-tale galvanometer hard over.

2 a.m. N.F. sending a message, but impossible to read it; oscillations frightful; can make out no intelligible word, only a letter here and there at intervals.

2·30 a.m. After numerous repetitions completed message, and repeated it back for safety.

3 a.m. Very strong earth-currents—spot goes off scale when N.F. turns switch.

3·15 to 3·45 a.m. No oscillations or kicks, but earth-currents very strong; spot still goes off scale when N.F. turns his switch.

4 a.m. Vibrations now very slight. Earth-currents gone off, working easier, but notice that when land office commences to send spot begins to oscillate, and disturbance gradually increases in strength. During very great disturbance got land-line disconnected altogether, but no improvement.

Sept. 28, 9 a.m. Spot steadier during the night.

Oct. 5, 10·30 a.m. This evening, while nothing doing on land-line mirror, spot pretty steady. Vibrations and jerks began to get worse directly land instrument worked.

Oct. 11, 4·23 p.m. "Hawk" having repaired '66 cable spoke to N.F. through it—signals first class.

7 p.m. Mr. Graves joined up an earth at Foilhommerrum to 5 miles of line wire, on poles to be used for working the land wire to London; this has remedied vibrations on cable instrument, but arrangement only temporary.

Oct. 15, 12·30 to 4·20 a.m. Great vibration preventing reading; obliged to stop cable while land-lines working.

8 a.m. to noon. Vibrations very bad.

6 p.m. Mr. Graves joined up another temporary earth at Foilhommerrum (F.M.), and found it made spot steady when land-lines not at work.

Oct. 27, 2·55 a.m. Vibrations and kicks bad all night; now obliged to stop land-line while we receive on cable.

3·54 a.m. Stopped on cable while sending to L.Y.

4·20 a.m. Somewhat less disturbance; working cable and land-line simultaneously, but with difficulty.

5·15 a.m. Again obliged to stop sending while receiving.

8 a.m. Vibrations gradually reduced during last three hours.

5·30 p.m. Joined up the second cable in place of earth, and worked by metallic circuit independently of the earth ; spot perfectly steady and working first rate.

Oct. 28, 4·15 a.m. Spot steady all night.

4 p.m. During the day tried working with one cable insulated at N.F., instead of being joined in metallic loop, using it for earth for V.A. and N.F., using his usual earth. Signals good and spot steady.

Oct. 29, 14·5 p.m. As an experiment, tried bringing all wires into land room direct from outside building, and connecting up as much in cable room to avoid possibility of induction. Result same as before, proving cause of vibrations to rest with the earth connections.

2·50 p.m. Resumed metallic circuit for cable ; working first class.

Oct. 30. Further experiments with different earths.

Oct. 31. Ditto ditto.

Nov. 1. With ordinary earths vibrations same as before.

Nov. 2. Ditto ditto.

Nov. 3, 4·30 a.m. Vibrations too strong to read ; obliged to connect up metallic circuit again, then quite steady.

10 a.m. Put on ordinary earth ; vibrations back again.

1·30 p.m. Returned to metallic circuit ; spot steady.

Nov. 4 to Nov. 13. Worked 1866 cable with 1865 cable for earth at V.A. insulated at N.F. end—N.F. using his usual earth ; signals good.

Nov. 14, 11 a.m. Worked 1866 cable with an earth made by means of a wire on poles, 5 miles, joined to iron of 1865, shore end ; signals with this arrangement were perfectly steady.

4·30 p.m. Divided 1865 land cable from 1865 sea cable at F.M., and joined the land cable to the iron of shore-end for earth. The line and earth underground cables being in the same iron pipes, for five miles. Worked 1866 cable with this arrangement with perfectly steady signals, and thus applied a permanent remedy for kicks and vibrations.

In the following July (1869) two more land-cables were laid for working the second cable, and when completed, answered perfectly.

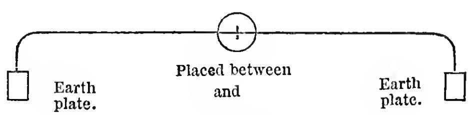
In 1873 four more land-cables were laid between the old and new stations in the same trench as those for the 1865 cable. These are now used in pairs for working the 1873 and 1874 cables. The whole of these cables can be worked now simultaneously, without any disturbance in the shape of induction, kicks, oscillations, or vibrations.

From the foregoing tedious details it will be seen that the disturbance from vibrations was first observed a few days before the discovery was made that the 2-mile earth-cable used for working the land circuits was broken, and no arrangement of earth-plates could be found to remove the vibrating disturbances at the old station.

The new station at that time being nearly finished, it was hoped that the separate earths provided there might remedy the evil, but it was found, on the contrary, that it was aggravated, and at times was so bad that the cable and land-lines could not be worked simultaneously.

This was, no doubt, more especially the case when earth-currents in the cable were strongest, and it would be interesting if the magnetical records for the days when the most disturbance took place should be found to confirm this view.

Before leaving the subject, it may be as well to chronicle the effects observed at the time upon a mirror galvanometer placed in circuit between the different earth-plates, as per plan, while either the cable key or the land-line key was worked, using for the cable, Earth-plate No. 2, and for land-line, Earth-plate No. 4.



GALVANOMETER.

Galvanometer between Numbers	Key Working.	Effect observed on Galvanometer.	
1 and 2	Cable	Vibrations Strong	
	Land-line ...	„ Strong	
	Neither ...	„ Steady	
1 and 3	Cable	„ Slight	
	Land-line ...	„ Strong	
	Neither ...	„ Steady	
1 and 4	Cable	„ Very strong	Greatest disturbance
	Land-line ...	„ do.	
	Neither ...	„ do.	
1 and 5	Cable	„ Slight	
	Land-line ...	„ Strong	
	Neither ...	„ Steady	
2 and 3	Cable	„ Strong	
	Land-line ...	„ Slight	
	Neither ...	„ Steady	
2 and 4	Cable	„ Very strong	
	Land-line ...	„ do.	
	Neither ...	„ Strong	
2 and 5	Cable	„ Strong	
	Land-line ...	„ Slight	
	Neither ...	„ Steady	
2 and {3, 5} joined.	Cable	„ Moderate	
	Land-line ...	„ Slight	
	Neither ...	„ Steady	
3 and 4	Cable	„ Strong	
	Land-line ...	„ Very strong	
	Neither ...	„ Strong	
3 and 5	Cable	„ Steady	Least disturbance
	Land-line ...	„ Very slight	
	Neither ...	„ Steady	
4 and 5	Cable	„ Strong	
	Land-line ...	„ Very strong	
	Neither ...	„ Moderate	

JAS. GRAVES.

The following Candidates were balloted for and declared duly elected :—

As Foreign Members :—

George C. Maynard	701, Fifteen Street, Washington.
Valdemar Lorentzen	Copenhagen.
P. C. Rasmussen	Fredericia, Denmark.
L. Sophus Schonheyder	Nykjöbing, Denmark.
Carl A. Petersen	Fredericia, Denmark.
Julius Neilsen	Copenhagen.
Major Axel Akrell	Stockholm, Sweden.
Th. Tetens Thaulow	Copenhagen.
J. E. Schmidt	Nyborg, Denmark.
Capt. C. H. Arendrup	Royal Danish Engineers, Copenhagen.
B. Bogislaus Carstens	Rónne, Bornholm, Denmark.

As Members :—

John M. Dunlop	Holebird, Windermere.
Col. Milman, R.A.	The Cloisters, Eton College, Windsor.
Charles G. Morgan	18, Cheapside, E.C.
Major St. John, R.E.	55, Parliament Street.

As Associates :—

W. A. Killingbeck	W. T. Henley's Telegraph Works, North Woolwich.
F. W. Riemenschneider	G. N. T. Co., 7, Great Winchester Street Buildings.
Christian Dreesing	G. N. T. Co., 7, Great Winchester Street Buildings.
Charles J. Simmons	106, Waddington Road, Kentish Town.
A. F. Clement	N. B. Railway, Edinburgh.
C. J. Griffith	Elm Lodge, Cheltenham, Gloucester.

As Students :—

J. Boyes	66, Old Broad Street.
J. Mehrtens	W. T. Henley's Telegraph Works, North Woolwich.

The Meeting then adjourned.

The Thirty-third Ordinary General Meeting was held on Wednesday, the 24th February, 1875, Mr. Latimer Clark, President, in the Chair.

The President announced that the Ronalds Library was now practically in possession of the Society, and that the Council had a considerable portion of the catalogue, which was a very voluminous document, and the books would soon be at the disposal of the Members. The Council proposed to send a van to Battle and have the collection brought to the Society's rooms.

The following paper was then read—

ON INDUCTION BETWEEN SUSPENDED WIRES AS AFFECTING AUTOMATIC TRANSMISSION.

By R. S. Culley, Vice-President.

A well-insulated line of telegraph was erected by the Postmaster-General in the year 1871 from London to Holyhead in connection with a new submarine cable to Ireland. From the central station at the General Post-office to Paddington the wires were placed underground, thence overground on the Great Western Railway *viâ* Oxford and Shrewsbury, as far as Chester, and for the remainder of the distance on the main road. They run underground on the latter portion for a length of 6½ miles. The total length of underground was 11 miles.

	m.	yds.
Central Station to Paddington . .	4	457
Flint Post-office	0	110
Conway Bridge and Post-office .	0	848
Talybont to Llanfairfechan . .	4	514
Bangor to Llandegai . . .	1	570
Menai Bridge	0	909
	10	1648

The total length of the entire line from central station to cable-hut at Holyhead being 303 miles 418 yards.

London and Holyhead Line.

		m.	yds.
4 wires each, No. 4 gauge.	Central Station to Paddington	4	457
	Paddington to Birmingham	129	110
	Birmingham to Wolverhampton	12	594
	Wolverhampton to Shrewsbury	29	1562
	Shrewsbury to Chester	40	352
No. 8 gauge.	Chester to Holland Arms	67	707
	Holland Arms to Rhosgoch	12	30
	Rhosgoch to Cable-hut	8	126
		303	418

	m.	yds.
Underground	10	1648
Road	75	833
Railway	216	1457
Total	303	418

Several of these wires were and are now worked automatically at high speeds, and a considerable disturbance was frequently noted, which had the appearance of slight contact or leakage from wire to wire. A careful examination of the entire line was made, and many small defects removed, but the interference continued.

Advantage was taken of the breaking of the cable in August, 1874, to institute careful experiments in order to ascertain how far the apparent contact was due to induction and how far to leakage, and at the same time to investigate the cause of a difficulty in signalling automatically in the middle of the day, which had been observed on other lines even older than, and therefore not so well insulated as, the Holyhead line.

The investigation was conducted by Mr. Marson, of my office, who is well known as a most painstaking and accurate experimenter.

The following diagram shows the relative of the wires between London and Oxford. The two lowest wires on the side of the poles furthest from the railway, Nos. 206 and 208, were led down into the experimenting room at the General Post-office. Between London and Oxford 206 is fixed on a wooden arm 33 inches long, and 208 on one 24 inches long, which is fixed 12 inches below the 33-inch arm. All the arms on the poles, nine in number, are carefully earthwired to intercept leakage and conduct it to earth.

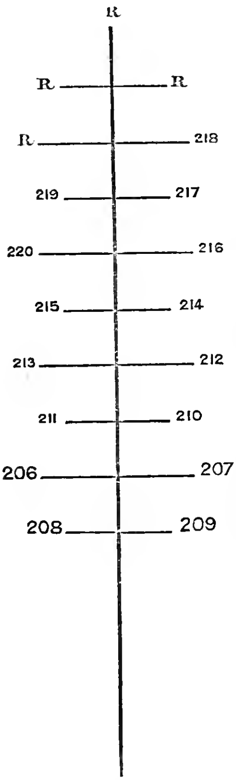


Fig. 1.

In the experiments made a "Bain" receiver was used to record the currents. It was fitted with two styles, insulated the one from the other, and the metal barrel over which the paper runs was also insulated. One style was connected to earth, the other to the wire under experiment. Thus + currents were registered by one of the styles, and − currents by the other. The paper was prepared with potassium iodide.

A—No. 206 was connected to the Bain apparatus, and was disconnected in succession at Chester, Birmingham, Oxford, Reading, and Paddington. If this wire were affected by the ordinary working currents on any of the other wires on the poles a mark would be produced on the paper. It was found that an effect did happen, and that it decreased in amount as the line was shortened, ceasing altogether when the wire was disconnected at Paddington, the underground portion only being left in circuit; showing that the result was not a consequence of induction between the buried

wires as would at first have been expected, but was due to some interference between the suspended wires.

B—No. 206 remaining as in A an ordinary single-current key was connected to 208, (see fig. 2), with 100-cells battery, so as to send positive currents to that wire; the Bain apparatus remaining on 206. Both wires were disconnected at Holyhead.

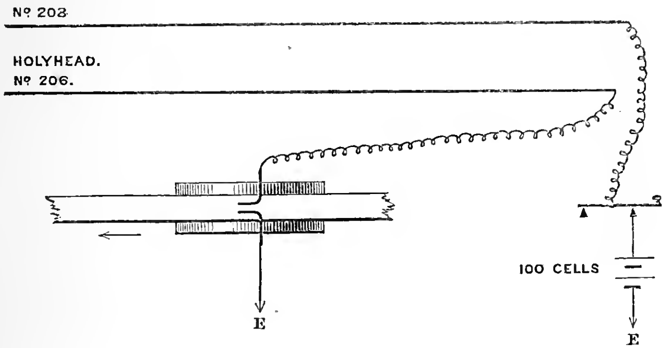


Fig. 2.

On depressing the key a short "comet-shaped" dash was marked upon the paper by the style connected with 206, and on raising the key a fainter but more elongated dash appeared at the style connected with earth, but no trace whatever was found on the paper in the interval between the depression and raising of the key, as would clearly have occurred had the mark been caused by contact or leakage from one wire to the other. The effect was obviously one of induction, and the shifting from one style to the other was caused by the change in the direction of the induced current, the style by which the + current entered the paper being that by which the decomposition of the iodide was effected. When the positions of the two line-wires were reversed the results were unaltered, except that the marks were reversed.

The marks from the two styles, when the back-stop of the key was disconnected, were as nearly as possible as in fig. 3.

Fig. 3.

C—When the "back-stop" of the key, instead of being disconnected as in fig. 2 B, was put to earth, distinct marks on the paper could be obtained as rapidly as the key could be moved; but when the back-stop was disconnected, as in fig. 2, the marks did not appear when the key was moved rapidly, owing to the currents in the inducing wire remaining too uniform to affect the induced wire.

D—The key and recorder arrangement remaing as in fig. 2, the two wires were disconnected and put to earth at Holyhead, Chester, Birmingham, Oxford, Reading, and Paddington successively. When the wires were insulated or disconnected at the distant end the marks were always tapering, and were gradually reduced in magnitude as the line was shortened, while when the wires were put to earth at the distant end the marks were square-ended dashes, not tapered, gradually diminishing in length and depth of colour as the line was shortened, somewhat thus—

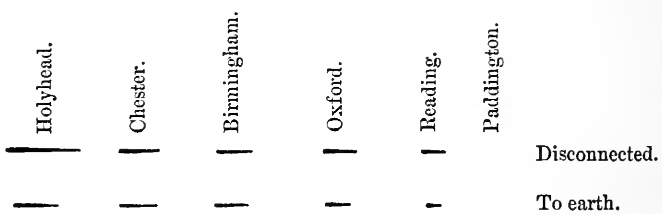


Fig. 4.

and disappearing altogether in both cases when the London underground section alone remained in circuit.

E—No. 206 was then connected to a speaking instrument, and 208 to the Bain apparatus. Signals being sent by Holyhead from a battery of 48 cells, the commencement and close of every signal was indicated on the paper by the inductive effect of the speaking wire on that connected to the Bain, but no trace of a mark appeared during the time the current was maintaining the signal, proving clearly the effect was not due to leakage from wire to wire.

One interesting and valuable fact which has eliminated itself by this investigation is the following:—

That wires on which double currents or reversals are used interfere much more with each other than when single currents are employed.

Thus: if two wires, 206 and 208, were disconnected at Oxford, and just sufficient power used to show a faint mark on the iodide paper connected with one of them, when the other wire was charged or discharged by a single current-key: the substitution of a double-current key with the same power brought out deeply-coloured marks of the usual comet or "tadpole" form from each style. This was always the case, and on this account the double-current key was used instead of the single-current in the subsequent experiments.

As our long lines are all worked by double currents or reversals, it was of advantage to test with the same arrangement.

This effect was noticed when using a Varley's wheel-key, so adjusted that in the middle position it puts the battery on short circuit, and simultaneously puts the line to earth. The newest form of key was then substituted, in which both battery and line are disconnected between the reversals; but the results were precisely the same.

As neither key had a middle fixed position, the following arrangement was adopted:—

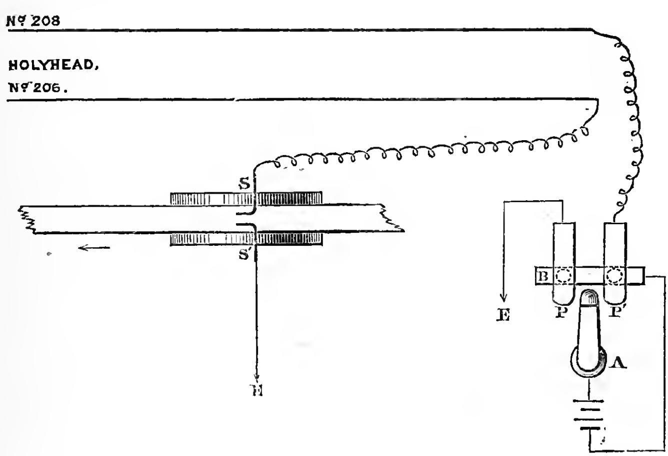


Fig 5.

Two springs P P^1 press upon the bar B; when the arm A slides under either of these springs it is connected with the spring which it raises, and disconnects the spring which it has raised from contact with the bar B. It thus performed the same as a Varley's key, with the exception that in its neutral position the battery was disconnected and the line was in connection with the earth. When the arm A was moved to the right and a positive current sent to the line, a faint mark appeared from the style S; when the arm A was removed to its central position, allowing the wire 208 to discharge, a corresponding faint mark appeared from style S^1.

When the operation was reversed by moving the arm A to the left the result was reversed. On moving A to the left a faint mark appeared from the style S^1, and when A was removed to the neutral position an equal and similar mark appeared from the style S. But when the arm A was moved rapidly from one side to the other the previously faint marks were transformed into well-defined tapering discharges.

The more rapidly the current is reversed, or, in other words, the less the interval allowed to elapse between one current and another in the opposite direction, the greater the effect inductively on the neighbouring wire. The increase of interfering effect when reversals are used can be readily understood.

When the two wires A and B were disconnected at Holyhead, A being in connection with a key, B in connection with double-style Bain apparatus, and the copper pole of a battery was connected with A, an induction current appeared on the style S; when the wire A

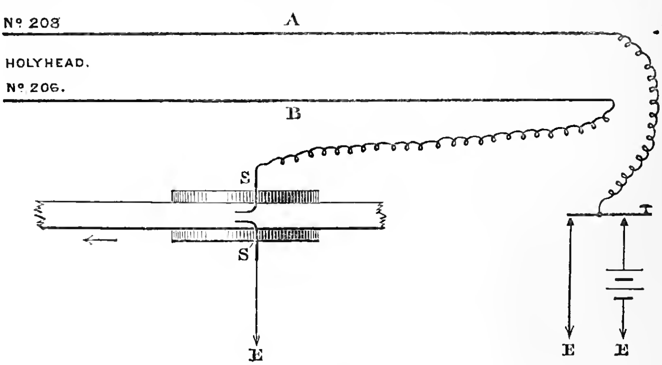


Fig. 6.

was discharged an induction current appeared on the style S^1; when the zinc pole of a battery was connected with the wire A a current appeared on the style S^1; and when A was discharged a current appeared from style S. So that when the current on A is reversed, instead of being simply discharged to earth, the two effects were added together and the induction considerably increased.

This of course applies to all apparatus, but it is rendered more perceptible to the eye on a double-style Bain apparatus.

F—Experiments were next made on the section London to Oxford, where the position of the wires beyond Paddington was accurately known, in order to find the effect of situation and distance. (See diagram 1.)

Nos. 206 and 208 were disconnected and the interference noted.

Nos. 207 and 209 were then disconnected, and the interference found equal to that between 206 and 208. This is just what it should be, as each pair of wires are similarly situated on opposite sides of the pole.

The induction between Nos. 206 and 207 was very feeble, only just visible. The induction between 208 and 209 was considerably less than that between 206 and 208, or between 207 and 209, but considerably greater than between 206 and 207, their relative distances being much less. Twenty-four cells or less were sufficient to show the induction between 206 and 208, or between 207 and 209.

G—Thus far the experiments had been conducted with the wires of ·240 inches diameter (No. 4 B.W.G.), as it was only between these thick wires that interference had been remarked in the transmission of messages. Nos. 211 and 213, which are of ·170 inches diameter (No. 8 B.W.G.), were disconnected at Chester and submitted to the same tests as the thicker wires. The induction between them was considerable, not equal to that between 206 and 208, but being nearly in the middle they were so acted upon by the surrounding wires that the same accurate observations could not be made as with the thick wires, which are fixed lowest on the pole.

Nos. 210 and 211 were tried and found to exercise much less influence upon each other than 211 and 213.

No matter which wires were taken, or what battery power was used, when any two wires were disconnected at any part of the line not the slightest trace of leakage could be obtained either in wet or dry weather.

This shows that the earth-wiring of the line is very perfect.

From these experiments it will be seen that where a number of wires are placed on the same pole the induction is greatest between wires on the same side of the pole and hanging one above the other; that this induction is more evident with the two lowest wires, as there is less interference from the others; that the induction between wires on opposite sides of the poles is less than it is between wires on the same side, because their distance is greater from each other; that the induction between wires on opposite sides of the poles decreases as the lengths of the arms increase. Consequently it may be assumed that in long overhead lines, where wires run together for a great distance, steps should be taken to increase the vertical distance between the wires.

As it might be argued that some specially sensitive instruments were necessary for this investigation, I should state, that, so far from this being the case, delicate instruments are of no use for the purpose. Galvanometers were discarded at the outset as being found, not only useless, but misleading. The astatic and ordinary were tried and rejected, and the Thomson is inadmissible on any land-line where there are more working wires than one on the pole; however shunted, the spot of light is too erratic to be of any service; it swings too long, and is useless to indicate rapid changes. While it is giving a quiver a relay is doing its work—that is, a relay properly adjusted. Moreover, induction currents require an apparatus which is ready to act, or which has, practically speaking, no zero to return to, which is the case in a Bain instrument. All galvanometers swing too much for observing rapid changes, and besides their magnetism is altered with every current passing through them. A great advantage in the use of a Bain is, that it not only gives a perfect record of all that occurs, but also shows accurately the strength of the current.

A Siemens' relay was found the best instrument in conjunction

with the Bain, being so manageable and the tongue being so easy of adjustment. It was an additional advantage to use a double-current key and an ordinary Siemens' relay for the experiments, as it reduced the apparatus to the same class as that used for ordinary working, preventing that objectionable difference between testing with special and working apparatus.

The Bain was not alone trusted in any of these experiments, but the Siemens' relay was always used in conjunction with it; the relay being substituted for the Bain at pleasure.

The effect on a relay was precisely similar to that on a Bain, only much sharper, on account of the resistance of the Bain paper; but the interference between any two wires under examination could not be sifted from the induction from other wires so well with the relay as with the Bain.

The induced currents were observed on the relay in the following manner :—

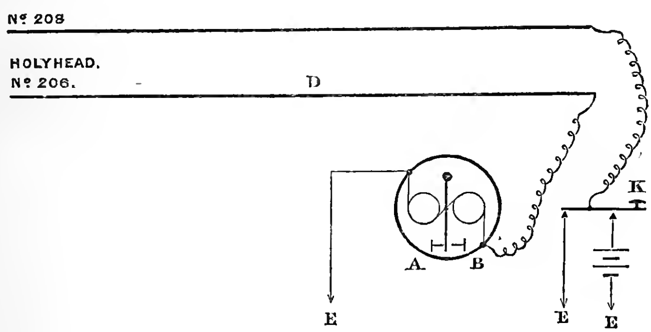


Fig. 7.

K may be either a double or single current key; the results with either are similar, but considerably more evident with the former than with the latter.

The relay was first set over to the side A so that a positive current coming from the wire D would move the tongue over to B. This occurred the moment the key was depressed, but although the key remained down the tongue immediately returned to A. When the key was raised so as to charge the wire negatively, the relay did not move, for the current induced on the wire connected with the relay only held the tongue more firmly against the stop A.

When the tongue of the relay was set over to the side B and the key depressed, the tongue remained on the side B, being held there more firmly by the induced current; but immediately the key was elevated the tongue went over to the side A for a moment, returning to B when the induced current had ceased.

When the tongue of the relay was set central or neutral the tongue moved simultaneously with the key, and would have recorded perfect signals on an inkwriter, appearing exactly like actual contact.

It is quite possible to obtain a double result: viz. contact combined with actual induction, and this no doubt frequently occurs; thus, if an artificial fault were put on the two wires under examination by joining a length of iodide paper between the two terminals to which the wires were connected, a continuous current appeared on one of the styles of the Bain's when the key was up or down, but the discharges were still equally apparent on the contact lines during the moment of change.

The foregoing observations were made on a comparatively new line on which the insulation is consequently above the average. On older lines the induction, though quite apparent, was not so great.

As has already been stated, no inductive action could be traced between wire and wire in the underground pipes between the central station and Paddington, although they are 4 miles 457 yards long. No effect was produced even by as high an electromotive force as 100 cells as regards induction *between wire and wire*, yet a very marked effect was observed even with but 24 cells as between *wire and earth*, and greater than was observed with similar battery power when the wires were disconnected at Oxford. Nor is there any record that induction between wire and wire was noticed on the buried wires formerly existing between London and Liverpool, though the discharge to earth was very violent. Moreover, having occasion to ascertain the speed on a circuit from Lowestoft to Holland and back, by the courtesy of the Submarine Company, a wire in each of the two Zandvoort cables was used.

During this experiment, which lasted five hours, and was made

with iodide paper, no trace of induction from the neighbouring wires through which the ordinary traffic was proceeding could be observed.

This interference will tend to prevent a much higher speed being obtained upon wires hung so closely together as they must necessarily be on the main lines of England, and will account for the comparatively low speed obtained by Edison in his series of experiments on the postal wires when carried on during the day, as compared with the results on a single-wire line in America or even with night experiments here.

A similar result has been observed in France as respects the "Hughes" printing instrument, and the wires have consequently been placed on several lines at much greater distances apart.

We are taught that very little dependence can be placed on speed trials conducted at night or on Sundays when there are but few wires working, or on mere laboratory experiments.

Mr. Preece: The object of this paper is not to show that induction between wire and wire on overground lines is any new thing, but that its effects are so far novel as to produce what was not suspected, a considerable diminution in the speed of working long circuits on the automatic principle. As long as circuits were worked on the ordinary Morse system by key, a difference of one or two words per minute was scarcely appreciable, but when we come to work long circuits such as those between London and Dublin by Wheatstone's automatic instruments, and obtained from 80 to 100 words per minute, it was found almost a regular thing that every day as the morning passed, and the sun rose towards the middle of the day, the speed of working was reduced some four or five words per minute. At first the reduction was attributed to the presence of earth currents, which are always present and which attain a maximum generally towards the middle of the day, and we were so far contented with that explanation; but when these further disturbances took place on

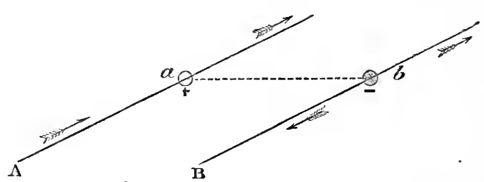
new lines which were exceedingly well insulated, and when the effect became variable with different conditions of the atmosphere and a reduced speed even more than we felt before occurred, then it became necessary to carry out a series of careful observations to endeavour to trace out the cause. The effect was, as Mr. Culley stated, precisely similar to that of a contact or leakage between wire and wire, but it was quite impossible to conceive that mere contact alone between two wires had the effect of reducing the speed of working. So Mr. Marson, in the way detailed in this paper, traced it directly to induction between wire and wire. The effect of that induction has been to reduce the speed of working between London and Dublin from an average of about ninety words a minute to something like eighty-five. This peculiar action between wire and wire is very observable on foreign lines in dry climates, and I have no doubt some gentlemen are present who will be able to give us their experience of the working in hot climates. For instance between Bombay and Madras there were two wires running side by side a distance of 800 miles, and it was found that they so disturbed each other from this fact that the position of the wires had to be changed. Again, between Teheran and Shiraz, in Persia, there are two wires running side by side, the one used for the through Indo-European traffic, and the other used for local purposes between Shiraz, Ispahan, and Teheran. It happened that some two years ago we held a *soirée* at the Albert Hall, and on that occasion I had to lecture before a very large audience; and, in order to illustrate the lecture, we had a wire brought into the room in connection with the Indo-European Line, and we succeeded in speaking to Teheran and Kurrachee. I have a brother in charge of one of the stations in Persia, and as we were talking in London about nine o'clock in the evening, which was their two o'clock in the morning, he was aroused by one of his clerks who said that a message was coming out in his name. Now the wire itself (this through Indo-European Line) does not enter the station where he was (I think he was at Shiraz), but the local wire does, and the inductive effect from the through wire upon the local wire was such that the signals sent by the through wire

were registered by induction on the local wire. In order to convince me that we were really through to India (many of us know that there is sometimes a good deal of hanky-pankyism done in public lecturing) the slip itself was sent me on the next day. I was once lecturing in Southampton and instead of speaking to Amsterdam, Berlin, and Vienna, as I thought, I was only, after all, speaking to "T. S." The fact unfortunately came out in this way. One of the audience in the hall wanted to know what time it was in Vienna when we were supposed to be talking to that place. The individual personating Vienna, knowing there was a difference of some forty minutes, instead of putting on the time took it off, and I was thus convicted at once before a Southampton audience as a swindler (laughter). However, I was as much deceived as my audience. In this particular instance a positive proof was sent me that we did speak direct from the Albert Hall to Kurrachee. Here is the identical slip that was taken off the instrument, and the marks are made not by the through current, but by induced currents upon the local wire as investigated in this paper.

Mr. Adams: I think the means adopted for obtaining these currents are novel and instructive; but I should like to have seen a relative value given to the induced current received. I have noticed such currents; but in this country the inductive effects described in this paper are somewhat new to me. And when we consider that the atmosphere of this country is very damp, one would think that all induction between two wires would be annulled by that moisture. It appears that this is not the case, although it is the first time I have seen it proved. As regards foreign lines I remember that during the first year of the Indo-European Line it was found impossible to work the two wires simultaneously, and I proved to my own satisfaction that the cause was nothing but induction. The line is between 750 and 800 miles, extending from Teheran to Tiflis; there is a relay station intermediate, notwithstanding which, it was found impossible to work the two lines. We could not attempt it with automatics. When the lines were put direct at the relay station and we had

800 miles of line the inductive effect would be very great, and not only fully deflect the galvanometer needle but also bring down the armature of the recording instrument. I think that experience will prove the possibility of induction between two wires. Still I think it quite unexpected that this should be found the case between wires in this country on account of climatic influence. I should add that in the case I have mentioned the difficulty was afterwards overcome by a general shift of insulators.

Mr. Preece: I will endeavour to point out to you, as clearly as I can, what really are the causes that lead to this peculiar disturbance between wire and wire, because they are of a nature considerably different to those which we experience in underground wires and in submarine cables, and which produce retardation of signals in those cases. Now, if we look back to the primary and elementary cause that leads to induction of all kinds, we find that it is extremely simple. Suppose we have any insulated body charged, say positively, with electricity, that body establishes in its neighbourhood what is called "an electric field." According to Faraday, this field is permeated in given directions with lines of electric force, and if any other insulated body be brought into that field, then the inductive effect of this charged body on the other body brought into the field is to polarise it—that is, to produce in it that condition in which one side is negatively electrified and the other side positively electrified. Let us now place this second body in connection with the earth, then we reduce at once the positive electrification on the opposite side to nothing, and we leave the body really negatively electrified, or in the opposite condition to which the inducing body is charged.



Supposing we have two wires A and B running parallel to each other, side by side, and if we assume that a current is flowing in

the positive direction in the one wire A, and if we take any point *a* (it does not matter where) and assume that to be our first body charged with positive electricity, then a point *b* on the other wire in the same place and in the field of *a* will be negatively electrified, because it is practically in connection with the earth. Now, this very curious fact occurs: assuming, in the first instance, that the point (*b*) of the induced wire is polarised and that one side is negative and the other is positive, then that point is in connection with the earth by two lines—one through the home station and the other through the distant station, and, having those two paths to reach the earth, it flows in two directions, and the result is that there are two currents in opposite directions in the induced wire. The current comes back to the home station in the *reverse* direction to the primary current, but the current in the other direction comes out in the *same* direction as the primary current. So that we conclude that all wires of this kind have currents flowing out at opposite ends and in different directions, and at some point there is zero or no current at all. The effect of this condition along the whole wire is that every element of negative electricity holds bound (to use an old term) a corresponding quantity of positive electricity on the other wire, and the result is that this element of positive electricity being withdrawn from the primary working current the initial strength of that working current is reduced, and thereby the speed of working diminished, so that when we have a wire surrounded by a number of wires all in the same field, the retarding effect of all these wires upon the first wire must be cumulative, that is that the retardation must increase with the number of wires, and in the same way it must increase with the contiguity of the wires, as shown in the paper. There is another fact that is mentioned in the paper and that is the influence that the earth has in effecting this induction. In the case of the underground wires between Telegraph Street and Paddington no effect of any sort or kind was observed. In all our cables, where four, five, or six wires are coiled up together, we have none of the induction between wire and wire, and the explanation of that is excessively simple. It is because when we place—take the body I supposed to be

charged positively and the other body I supposed to be charged inductively—the earth between these two wires we entirely destroy the field as far as the one side is concerned. In the case of wires passing through underground pipes, as they do between Telegraph Street and Paddington, we have our wires generally full of water. We have them surrounded thus by the earth, and the conditions of the electric field which exists in overground wires, and which produce those effects which have been mentioned, entirely destroyed. It is also mentioned that a double current working the reverse current, that is a positive current at once succeeded by an inductive current, increases the effects. It does as far as the effects are observable upon our instruments at two extremes, but it does not at all follow that because the currents are strengthened at the two ends that the retarding effect upon the wire is increased. On the other hand I am inclined to think that the same influence which the double current working has in submarine cables or underground wires in increasing the speed would be precisely the same. Thus, you see, by simply working up from a simple experiment which has been tried, we are able to follow not only the inductive effects observed upon an overground line of this kind, but also every case of so-called dynamic induction. If a conducting body be brought into an electric field and there be connected to earth, you will always charge that body with an electricity opposite to that which the charged body has. This I take to be the simple explanation—if I have made it simple. It is rather a difficult thing without the aid of apparatus or diagrams and with only a black board in the shade to illustrate facts, but if I have been clear I think I have succeeded in explaining the *rationale* of this phenomena and in showing that with well insulated lines and dry atmosphere they must necessarily happen, and if they so happen they must necessarily lead to the reduction of the speed of working. At present we have not arrived at any other means of getting over the conditions than by putting the wires as far apart as we can; as long as wires are maintained as they are now there must be this action between wire and wire.

The PRESIDENT: I should like to ask Mr. Preece if he has tried

the experiment on more distant wires, and what he was led to suppose as to the advisability or not of having more distant arms?

Mr. Preece: We have tried the experiment as far as we have been able, and find that as we increase the distance the effect diminishes very rapidly.

A Member: Do you ever meet with the difficulty practically in any system of working besides the automatic principle?

Mr. Preece: No, never, except in the case of the automatic, though we have always found traces of what we considered to be leakage or contact on all wire lines, and we always thought it was a little amount of contact which the earth wires did not succeed in removing.

A Member: What amount of induction is there between lines 211 and 208?

Mr. Preece: It is almost solely a question of the relative surface of the wires opposed to each other.

A Member: Between 208 wire and 206 wire is there any difference?

Mr. Preece: No; practically none, provided the wires remain about the same size, because the inductive capacity and resistance are not sufficiently great to show a difference in the case of overground wires.

Lieut. Jekyll, R.E.: A very striking instance of induction occured in underground wires under my observation. It was in connection with siege operations at Chatham. We had four submarine mines in the Medway which were all furnished with independent cables buried in the same trench and leading to a fort distant about three-quarters of a mile. On endeavouring to fire the furthest one by means of a battery we failed, so a frictional machine was applied, and, to our great surprise, instead of firing that mine the three others exploded simultaneously. We afterwards found by repeated experiment that we could always fire an electric fuse on a wire lying in the same trench with another to which the electric current was applied.

Mr. Preece: Were the wires in the same casing?

Lieut. Jekyll, R.E.: They were india-rubber casings and lay in the same trench.

Mr. PREECE: I could not explain that on inductive principles, I should conceive it is due to leakage.

Lieut. JEKYLL, R.E.: It certainly did not occur through that.

Mr. PREECE: Then it is an anomaly which I do not quite understand at present. I should say if there are four india-rubber or gutta-percha wires laid in a trench surrounded by water and earth there could be no such effect. If the earth in the trench were very dry, practically there would be no earth at all, and it might occur; but if the trench were wet there could be no such inductive effect between wire and wire as that which has been explained.

Lieut. JEKYLL, R.E.: By means of induction the effect could be recorded on another wire. I should like to know the kind of instrument on which the message was recorded.

Mr. PREECE: I believe it was Siemens' polarised relay, but whether worked by continuous currents or by a short first current and then reversed I do not know. Perhaps Mr. Adams, who was out in Persia, might know how they worked that wire.

Mr. ADAMS: Polarised relays of the ordinary kind, double currents.

The PRESIDENT: We have seldom listened to a more interesting paper, or one more clearly described than that we have had before us this evening. I think the first person who spoke distinctly about induction in telegraph wires was Sir Francis Ronalds, who in his book expressly foresaw the difficulty that might arise from this cause, and said that even if the time necessary to transmit a signal from London to Brighton were a minute, that would not be an insuperable obstacle to telegraphy. Subsequently, Sir Charles Wheatstone made his memorable experiment on the velocity of electricity, and obtained a speed which was at first supposed to be the normal velocity of transmission of electricity. Faraday subsequently pointed out that this speed was doubtless affected by induction of the neighbouring walls of the room and wires, and felt no doubt that had the room been smaller, or the wires differently placed, the velocity obtained by Sir Charles Wheatstone would have been different. In fact, he thoroughly appreciated the nature of induction even in those days.

Now with regard to the paper, one of the first observations that strikes one is that we cannot help complimenting the Government service on the very perfect insulation they have attained in their overground wires, for unless these wires had been very perfectly insulated the effects of induction between wire and wire would have been invisible. In the olden days we were perfectly aware that such induction existed, and I among others made many experiments to make its effects visible, but owing to the imperfect insulation of our wires and the imperfection of our instruments I was never able to observe any effect, although I was sure it was there. I even went to the trouble of suspending and insulating some hundreds of square yards of calico on two parallel wires at a distance of twelve inches apart, and although I failed to perceive the inductive influence I felt the failure of the experiment was owing to the inadequate apparatus I used for the purpose. Now the induction as shown by these experiments is evidently much stronger between wire and wire than between wire and the earth. The laws of induction are perfectly known and are stated in electrical treatises, so that the relative degree of induction might be calculated by any one, and doubtless that calculation would agree with the results obtained, but it is interesting to observe how accurately Mr. Culley has found the amount of induction between nearer and more distant wires and between wires of different sizes to agree with what it obviously should be in theory. Another point proved by these experiments is that wires of large size (No. 4) although they have great advantages, inasmuch as they transmit a much more powerful current of electricity than the smaller wire ordinarily used, still it is evident that they are not so good in one respect, inasmuch as they both receive and create more induction in neighbouring wires. Now this is an important subject—more so than at first sight appears. No doubt as the demand for telegraphy increases on the part of the public and automatic transmission is more employed, we shall meet this difficulty more and more every day, and therefore it is a subject well worthy our consideration whether there is not some means of obviating, or at least reducing, this induction. The first and obvious consideration is that we should

place the wires as far apart as possible, and mix them about one among another, so that no two wires run parallel for any great distance, and Mr. Culley has himself told me that he contemplated that in the event of their requiring to obtain the very highest speed by automatic transmission it would be necessary so to mix the wires about—to let the current run a part of the distance on one wire and a part on another, and so keep varying the distances between them that the average influences of the wires upon each other would be very much reduced. The only difficulty I see is a small one, viz.: That in testing the wires the alternations of the numbers would be more frequent than at present.

It appears to me that by thus varying the positions of the wires you may practically obviate the evil influence of induction from wire to wire, which is our only bar to high speed automatic transmission. I may say that I quite agree with the remarks made by Mr. Preece, and I do not believe that two wires placed in moist earth can have induction from one to the other, and the effect which Lieutenant Jekyll observed of fuses being fired by the presence of currents in the neighbouring wires could not have been due to simple induction. I can imagine that the primary wire which received the charge would cause an electrical tension on the surrounding ground. For example, supposing the wire were charged positively it would expel from its external surface a certain positive charge, and that would tend to distribute itself throughout the earth. It would have no special tendency to run to the neighbouring wires, but it would merely seek to equalise itself throughout the earth. In doing so it would radiate in every direction, and if a neighbouring wire were within its influence it no doubt would momentarily raise the tension round that wire and cause a minute induced current to be discharged from it. Still I do not think in moist earth it would be easily possible to show that effect, and I cannot help thinking that either the wires were in very dry ground or sand (when such an effect would be easily produced) or else that there was some accidental contact between the wires.

Between London and Liverpool, a distance of some 210 miles,

eight underground wires were laid in 1853; when they were first laid down they were in excellent order, and the ordinary effects of induction were beautifully illustrated by them. We made a great many experiments on the effect of induction between wire and wire, and with the instruments we then had in use we were quite unable to perceive any induction from one to another. Even when we charged four of the wires suddenly with a strong battery power and put our receiving instruments on the remaining four, we could not get any sensible discharge. Therefore I cannot think that a few hundred yards of wire, even with the very powerful tension caused by an electrifying machine, could have had the effect of igniting the fuse. It is true that subsequently in the Wexford cable, with instruments of the most sensitive kind, I have witnessed a sensible induction from wire to wire. That current, I imagine, was caused in the manner I have indicated: that the sudden positive electrification of one of the wires threw off a small quantity of positive electricity from its exterior, and that electricity, before it could distribute itself from the cable into the sea, would momentarily raise the tension of the whole core of the cable and cause an inductive discharge.

I was very much pleased this evening with the careful way in which these experiments were made, and it now only remains for me to move that a vote of thanks be presented to Mr. Culley for the able and interesting paper he has read to us this evening.

The following Candidates were balloted for and declared duly elected :—

As Foreign Members :

Lieutenant F. Dreyer .	Royal Danish Navy, Shanghai.
Lieutenant E. Suenson .	Royal Danish Navy, Copenhagen.
Don Manuel Ramira .	Antonio Tomas, Argentine Republic.
Don Luis Zignago . .	Frederacion, Argentine Republic.
Don Raphael Gimenez .	Santiago, Argentine Republic.

As Members :—

George Dering . .	Lockleys, Welwyn.
John Fuller . . .	116, Fenchurch Street.
Bennet Pell . . .	Singapore.

As Associates :—

Conrad Cooke . .	14, Marlee Terrace, Clapham Rise.
George Fuller . .	116, Fenchurch Street.
Sholto Douglas . .	Singapore.
Major Puget . . .	34th Regiment, Carlisle.
Henry Starke . .	Government Telegraph Department, Brisbane, Queensland.
R. Price Williams . .	Great George Street, Westminster.
Jacques Ducloy . .	Submarine Telegraph Company.

The Meeting then adjourned.

ORIGINAL COMMUNICATIONS.

DUPLEX TELEGRAPHY.

CHEMNITZ (SAXONY),
12th November, 1874.

As the high reputation of the Society of Telegraph Engineers must give to its transactions and publications a corresponding importance, so does it become the duty of the Members collectively to see that, in the communications upon any transaction, no point may escape which requires a further explanation. This consideration induces me to bring before you, honoured Sir, as a point of this kind, a matter which you will perhaps be good enough to allow me to explain further on, and to ask you to be so kind as to bring the same before the next meeting of the Society in order that it may be properly cleared up.

In the discussion upon Mr. Culley's paper which took place at the meeting of 28th January last under your presidency, Mr. Clark (as I learn from the Journal of the Society, No. VII. p. 26, which has just reached me) spoke as follows:—"It is only right to say the duplex system was known as long ago as 1853, when experiments were made by the Electric Telegraph Company and answered tolerably perfectly." This statement is so strikingly opposed to what has hitherto been held to be the history of the discovery of the duplex method, that unless the figure 1853 is a misprint (a lapse of memory on the part of Mr. Clark can hardly be supposed), it would, in my opinion, only be right for Mr. Clark to bring forward evidence in support of his statement. Should it be a misprint the mistake is one which it is incumbent on the Society itself to correct in a prominent manner.

I have in a detailed article printed in the *Journal Télégraphique* (Vol. 2, Nos. 29 and 30), and in a nearly similar article in *Dingler's Polytechnischem Journal* (Vol. CCXII. p. 3), endeavoured to correctly set forth the claims of Messrs. Vaes Stearns, W. H.

Preece, and Winter. I send to the Society a copy of the last article, and for the reasons therein given, and relying upon the proofs contained in my work *The Copying Telegraph, Type-Printing Telegraph, and Double Telegraph* (published by B. G. Trübner, Leipzig, 1865), I had, till they were contradicted, maintained that the discovery of the duplex method was a German one.

If, however, Mr. Clark's statement has reference to Mr. W. H. Preece's duplex method stated to have been invented in 1855 (*Telegraphic Journal*, No. IV. p. 60), and tried in 1856 (*Telegraphic Journal*, No. XVI. p. 277, in partial contradiction to No. IV. p. 60), I may remark that this does not appear to me to be sufficient foundation for the fact, seeing that Mr. Preece simply states that owing to the unfavourable result he published nothing about his method.

In conclusion, I would add that it would afford me much pleasure to receive a few lines from you in acknowledgment of this letter, and intimating your willingness to accede to the request I have made in the first part of it. Meanwhile I do not fail to assure you of my greatest esteem as

Your most obedient,
(Signed) Prof. Dr. R. E. ZETZSCHE,
Foreign Member S.T.E.

Professor Sir WILLIAM THOMSON, F.R.S.
President of the Society of Telegraph Engineers, London.

Prof. Dr. R. E. ZETZSCHE,
DEAR SIR,

March 22nd, 1875.

I am sorry to learn from Mr. Preece that your letter of the 12th November to Sir William Thomson, F.R.S. the late President of the Society of Telegraph Engineers, remains still unanswered. As it was addressed to him I had supposed that he had replied to it at the time.

The statement with respect to the invention of Duplex Telegraphy which is reported in the Journal of the Society, No. VII. page 26, was made from memory only, but still it is I believe substantially correct. I have always understood that the duplex

system of telegraphy was invented by Dr. Gintl. His system became known to me in 1853 when I was the Engineer of the Electric and International Telegraph Company, and Mr. W. H. Preece, who was at that time my Assistant-Engineer, made at my request many experiments upon the system, which, as I stated, answered very perfectly; but we had at that time plenty of wires and had no need of the duplex system.

In the following year, 1854, Mr. R. S. Newall patented some improvements on Dr. Gintl's system (Patent No. 2308, 1854); and a little later on, the 8th November, Mr. C. W. Siemens also took out a patent on the subject (No. 2,366, 1854); both these were for improvements on Dr. Gintl's system. I am not aware that Mr. Preece has ever invented any system of Duplex Telegraphy. My remarks were as follows:—"It is only right to say the duplex system was known as long ago as 1853, when experiments were made by the Electric Telegraph Company and answered tolerably perfectly," and I think you will admit that there is nothing in that statement but what is in perfect accordance with the facts of the case. I did not state that Mr. Preece had invented the system or that it was any invention at all, but only that it was known as long ago as 1853.

On referring a second time to my remarks I find that I am reported to have said that, "at the time Messrs. Gintl and Siemens made improvements in the system." This is incorrect. I intended to say and I have no doubt really did say Messrs. Newall and Siemens made improvements in the system.

In this country Dr. Gintl is universally considered to be the first originator of Duplex Telegraphy.

I am, dear Sir,

Your obedient servant,

LATIMER CLARK,

President of the Society of Telegraph Engineers.

CHEMNITZ (SAXONY),
11th May, 1875.

I had great pleasure in receiving your letter of the 22nd of March at my return to Chemnitz; the enormous amount of work left to me during my absence did prevent me till now to express to you my most sincere thanks for your kind and very explicit reply to my letter of the 12th November, 1874. Believing, as you told me, to give my letter of the 12th November, 1874, and your reply to it of the 22nd March in print to the Journal of the Society, I should recommend to add also the following remarks. As regards Dr. Gintl's invention of reciprocal speaking, I beg to remember Dr. Werner Siemens' remarks (*Poggendorff's Annalen*, Book 98, page 120), as I have been informed from truthworthy parties in Vienna that the idea of reciprocal speaking has been communicated to Dr. Gintl by Dr. Petrina, Professor of Physic at Praag. It must further not be left out of sight that Mr. R. S. Newall bought Mr. Frischen's invention, which contract is still existing, and who then took a patent out in the year 1854, as I already mentioned in the *Journal Télégraphique* (II. Bd. page 456), and have also done so before in my pamphlet *The Copying Telegraphs*, &c. page 117. The experiments of reciprocal speaking made in England could of course only be mentioned "in January, 1855."

I have the honour, Sir, to be
Your obedient servant,
(Signed) Dr. ZETZSCHE.

LATIMER CLARK, Esq.
President of the Society of Telegraph Engineers, London.

INDIAN AND AMERICAN TELEGRAPHS.

In No. VII. of the *Journal of the Society of Telegraph Engineers*, I observe on page 115 a letter written by Mr. D. Brooks, dated Philadelphia, August 21st, 1874. As this letter contains some criticisms of parts of my paper on "Some Points in Connection with the Indian Telegraphs," it is necessary for me to enter again into the matter. In my paper, the question, What would produce the insertion of one per cent. of bad insulators of a certain resistance on a line of a definite length? was investigated mathematically, and the result arrived at was, that in the case under consideration the consumption of battery material would be increased 60 per cent., and the received or effective current diminished 20 per cent. Mr. Brooks objects to this, informing us that, in addition to the battery material employed in producing the line-current, battery material is also used up when the poles are insulated. As, however, this local action is in no way connected with the good or bad insulators, it had absolutely nothing to do with the question I was considering, which concerned itself solely with the battery material legitimately employed in producing the line-current in the different cases considered.

Farther on, Mr. Brooks asks, "Are the benefits derived from the accurate quantitative testing of new insulations at the stores in Calcutta, Bombay, and Madras, commensurate with the care bestowed?" and quotes what I stated to be the average insulation of the lines in India to prove that the answer is in the negative. The average insulation given is that of the lines constructed in India during the last quarter of a century, whereas, the quantitative testing of the insulators was only regularly established in 1871, in consequence of the insulation of some of the lines being much lower than it would be if all the insulators were good. Mr. Brooks refers to some lines in America that have as much as a hundred millions per mile in rain. Lines constructed of Schönberg porcelain insulators (many thousands of which, not some thousand, as printed in the Journal) I passed for the Indian Government at the

end of 1872, should have (as can be deduced from the results of the tests, page 189 of my paper) an insulation of two hundred thousand millions per mile, if only the rims or edges of the porcelain cups were dry, the remainder of the cups being perfectly wet outside and inside, as in the testing. Even diminishing this result a hundred per cent. to allow for damp on the edges also of the cups, we should still have with Schönberg porcelain an insulation during rain twenty times as high as that of the best lines in the United States. As, however, I did not expect that Schönberg insulators solely would be used in the construction of any one line in India, but that they would probably be mixed with the cheaper insulators, either of Messrs. Pinder Bourne, or of Madame Defuisseaux, I anticipated in my paper a very much lower probable insulation, one in fact nearly as low as that of the very best American lines.

With reference to signalling, Mr. Brooks condemns the acknowledging tap given in India, by the receiving signaller, alleging as his objection that in order that this acknowledgment may be given the pen must be dropped at the end of each word. The acknowledgment is of course given with the left hand, and does not, therefore, materially interfere with the speed of working; consequently Mr. Brooks's inability to see "how two good operators could, by the Indian method, get more than twenty messages of twenty words each per hour over a line," will now be removed, and he will understand how that fifty such messages hourly is no uncommon number.

Mr. Brooks next refers to Duplex Telegraphy. For full particulars of what has been done in India under this head, I would refer to the papers published by Mr. Schwendler, in the "Journal of the Asiatic Society of Bengal," vol. xliii. Part II. 1874.

The most serious question, however, referred to by Mr. Brooks is the subject of the proper resistance for a relay. As he quotes a report of Mr. Varley's on American closed circuit working, and proposes applying the rules therein given to open circuit working, it is not surprising that he arrives at the conclusion that "doctors disagree, and that there is no subject upon which there are such

a diversity of opinions as upon the proper resistance of a relay." From this I conclude, therefore, that the mathematical analysis of the subject cannot be generally known, and in consequence I append an investigation, in which I have proved:—

I. That, with the system of working employed in India, high resistance relays are the best for long lines.

II. That, with the system of working employed in the United States, rather low resistance relays must be used for even long lines.

III. That, in consequence, closed circuit working is a far more expensive system than open circuit working.

IV. That the American system of using one-line battery only for several lines is far more troublesome and expensive than the employment of several line batteries.

To determine the best Resistance for a Relay.

1. Open circuit, sending current not working the relay of the sending instrument.

Let K be the true conduction resistance of the line up to the position of the resultant fault, that is the point where a single leakage could be substituted to replace all the leakages at the different points of the line.

Let k be the true conduction resistance of the rest of the line.

,, I be the total insulation.

,, C be the current passing through the receiving relay.

,, E be the electromotive force of the sending battery.

,, R be the resistance of the receiving relay

$$\text{then } C = \frac{IE}{K(I + k + R) + I(k + R)}$$

if we neglect the resistance of the battery.

Now if the size of the bobbins of the relay be constant as well as the specific resistance of the copper wire employed in winding, then neglecting the thickness of the insulating covering

$$R \text{ varies as } N^2$$

where N is the total number of convolutions of wire in the bobbins;

this is easily seen, since, if the number of turns be doubled, the length of wire will be doubled, and the sectional area of the wire halved, therefore the resistance will be quadrupled.

If the relay be polarised the delicacy Y } varies as CN

$$\text{,,} \quad C\sqrt{R}$$

$$\text{,,} \quad \frac{IE\sqrt{R}}{KI + (K+I)\,k + (K+I)\,R}$$

or if the relay be not polarised then Y } ,, C^2N^2

$$\text{,,} \quad \left\{\frac{IE\sqrt{R}}{KI + (K+I)\,k + (K+I)\,R}\right\}^2$$

In either case, equating $\frac{dY}{dR}$ to 0 in order to obtain the resistance that gives maximum delicacy, we have

$$R = \frac{KI}{K+1} + k$$

To use this equation we must take I as low as ever it is likely to be without the existence of an actual fault, since of course we wish the relay to be best suited to the line when the received current is the weakest.

For example: Let the line be 200 miles long, composed of wire having say 10. B.A. units per mile, let the leakage be uniformly distributed over the line, and the lowest normal insulation be, say, one million per mile,

then $$K = 1000$$
$$k = 1000$$
$$I = \frac{1000\,000}{200} = 5000$$

and $$R = 1833 \text{ B.A. units.}$$

If the line be exceedingly well insulated, so that practically I is infinite, then

$$R = 2000 \text{ B.A. units.}$$

2. Closed circuit, battery at each end, sending current passing through all the relays including that of the sending station.

First let us take the case of an exceedingly well insulated line so that I is very great, then neglecting the resistance of the batteries.

$$C = \frac{E}{L + R_1 + R_2 + R_3 + \&c.}$$

where E is the sum of the electromotive forces of the two batteries, L the total conduction resistance of the line, and R_1, R_2, R_3, &c , the resistances of the relays.

Now since I is very large the same current practically will pass through every relay, therefore there is no reason why the resistance of one relay should be larger than that of another, consequently we may assume

$$R_1 + R_2 + R_3 + \ldots = pR.$$

where p is the total number of stations :

Consequently with polarised relays the delicacy Y of any one relay $\Big\}$ varies as $\dfrac{E \sqrt{R}}{L + pR}$,

or with unpolarised relays Y ,, $\left[\dfrac{E \sqrt{R}}{L + pR}\right]^2$

in either case equating $\dfrac{dY}{dR}$ to 0 to obtain the resistance that gives maximum delicacy we have

$$R = \frac{L}{p}.$$

Example.—Let us take the same line as before with excellent insulation, and let p, the total number of relays in circuit, be 20, then

$$R = \frac{2000}{20}$$
$$= 100 \text{ B.A. units.}$$

If the insulation be not perfect, then the mathematical reasoning used in determining what should be the exact resistance of each of a number of relays, in order that the delicacy of each shall be a maximum, is not as easy as before, since now the same current will not traverse each instrument. The mathematical investigation gives, however, as a result, that the resistance of each relay should be somewhat less than the fraction obtained by dividing the total conduction resistance of the line, independent of the resistance of

the relays by the total number of relays in circuit, in other words should be somewhat less than the result previously obtained with perfect insulation.

Open Circuit Working compared with Closed Circuit Working as regards Battery Power, Consumption of Battery Material, &c. when the terminal stations are signalling one to the other.

According to the best method of open circuit working the current sent from station A to station F passes only through a galvanoscope at all the intermediate offices B, C, D, E; the resistance of the galvanoscope being the least comparable with the reading of the name of the station called. When a message is to be received by an intermediate station this galvanoscope is replaced by a relay. The most advantageous method of working a local line on open circuit would be obviously as follows: the relay at each of the terminal stations to have the resistance that gives maximum delicacy when the terminal stations are signalling one to the other, the batteries at all stations to be sufficiently strong to work the terminal relays satisfactorily; and then the relays at the intermediate stations may have resistances somewhat less than those necessary to give maximum delicacy when receiving from the terminal stations. With closed circuit working, on the other hand, no relay should have less than the resistance calculation shows will give maximum delicacy.

In the following consideration let the relays be polarised, all the magnets of the same strength, and all the bobbins of the various relays of the same size, but, of course, wound with different wire in the different cases.

Let the line, for simplicity of calculation, be very well insulated.

Let L be the true conduction resistance of the line wire only,

G be the resistance of each of the galvanoscopes employed in open circuit working,

p be the total number of stations in circuit,

Let e be the electromotive force of each cell,

v be the number of cells employed at *one* terminal station in open circuit working,

V be the total number of cells employed at both terminal stations in closed circuit working,

Then according to previous calculation regarding maximum delicacy we have, neglecting the resistance of the batteries,

OPEN CIRCUIT.

Resistance of terminal relay

$$= \mathrm{L} + (p-2)\,\mathrm{G}$$

Current, c, passing through terminal relay

$$= \frac{ve}{2\,\{\mathrm{L} + (p-2)\,\mathrm{G}\}}.$$

Electro-magnetic attraction, y, of terminal relay

$$= \mathrm{J}\,\frac{ve\,\sqrt{\mathrm{L} + (p-2)\,\mathrm{G}}}{2\,\{\mathrm{L} + (p-2)\,\mathrm{G}\}}.$$

$$= \mathrm{J}\,\frac{ve}{2\,\sqrt{\mathrm{L} + (p-2)\,\mathrm{G}}}.$$

CLOSED CIRCUIT.

Resistance of each relay

$$= \frac{\mathrm{L}}{p},$$

Current C, passing through each relay

$$= \frac{Ve}{2\mathrm{L}},$$

Electro-magnetic attraction, Y, of terminal relay

$$= \mathrm{J}\,\frac{Ve\,\sqrt{\mathrm{L}}}{2\,\mathrm{L}\,\sqrt{p}},$$

$$= \mathrm{J}\,\frac{Ve}{2\,\sqrt{p\,\mathrm{L}}},$$

where J is a constant depending on the forms of the bobbins, the amount of magnetism in the permanent magnet of the relay, &c., but independent of the wire with which the relay is wound; J is therefore a constant in this investigation.

Now, in order that y may equal Y,

$$J\frac{ve}{2\sqrt{L+(p-2)G}} = J\frac{Ve}{2\sqrt{pL}}$$

$$\therefore \frac{V}{v} = \frac{\sqrt{pL}}{\sqrt{L+(p-2)G}}$$

Also, if the cells be joined all in series, which is usually the case in line batteries, the amount of battery material consumed in sending the current is proportional to the product of the current into the number of cells employed, therefore if x be the battery material consumed in sending the current from terminal station to terminal station in open circuit working, and if X have a similar meaning in closed circuit working, then—

$$\frac{X}{x} = \frac{CV}{cv},$$

$$\therefore \frac{X}{x} = \frac{\frac{Ve}{2L}}{\frac{ve}{2\{L+(p-2)G\}}} \times \frac{\sqrt{pL}}{\sqrt{L+(p-2)G}},$$

$$= \frac{L+(p-2)G}{L} \times \frac{pL}{L+(p-2)G}$$

$$= p.$$

Now, in open circuit working, when intermediate stations communicate with one another, or when an intermediate station signals to a terminal station, the consumption of battery material will be less than when the two terminal stations communicate with one another, since the number of cells employed at an intermediate station will be so much less than the number employed at a terminal station, that the current received at the terminal station shall be always the same. On the other hand, the consumption of battery material at a terminal station will be greater when it signals to an intermediate station than when signalling to the other terminal station, since the current traversing the sending battery will be greater, owing to the external resistance opposed to it being less. This extra consumption might of course be avoided, by using different sending batteries at terminal stations, but the gain would be hardly worth the trouble required to change from one battery

to another. On the whole, therefore, in open circuit working the total amount of battery material consumed at all stations in producing the line-currents will be less than if all the messages were sent from one terminal station to the other. In closed circuit working the amount consumed is, of course, independent of the particular offices sending and receiving the message. Consequently we may conclude that, independent of local action in the batteries when they are not working, the total amount of battery material legitimately consumed in closed circuit working is rather more than p times the whole consumption with open circuit working, p being, as defined, the total number of stations in circuit.

Consideration regarding the Employment of one Line-Battery only, or of Separate Batteries for the different Lines coming into an Office.

If a station A be signalling to a station Z, whether by open circuit or by closed circuit, it is clear that constant working (that is working without perpetual adjustment of the relays) can only be obtained when the current leaving each station is tolerably constant. We have, therefore, only to consider how the constancy of the *sent current* is affected.

Let E be the electromotive force, and B the internal resistance of *one* battery used for working two lines.

Let K be the circuit resistance of the first line, that is the resistance that would be obtained on measuring when the line is joined up at the distant station as it usually is when receiving a current.

Let k be the circuit resistance of the second line.

Let C be the current sent on the first line when a current is *not* also sent on the second line.

Let c be the current sent on the first line when a current is *simultaneously* sent on the second line.

then
$$C = \frac{E}{B + K}$$

$$c = \frac{kE}{(K + k)\,B + Kk}$$

Now, in order that C may equal c which is the condition of constant working we have, for values neither infinite nor nought of K and k.

$$B = O.$$

Consequently in order to approximate to this Grove's cells, since they have a very small internal resistance, must be employed when one line battery only is used for different lines.

But the maintenance of a Grove's battery is exceedingly expensive and troublesome owing to the cost of the nitric acid and the rapidity with which it becomes diluted when the battery is acting. Consequently separate line-batteries of Daniell's, or other cells, are much to be preferred to one line-battery only.

Mr. Brooks' constant reference to his own insulator induces one to remark that the form of the insulating cup, as pictured in the advertisements, would, in a country where spiders and other insects are numerous, cause the whole inside of the insulator to be rapidly choked with their webs, and in addition would quite prevent the inside being easily cleaned.

W. E. Ayrton.

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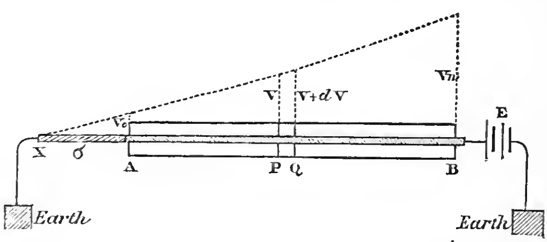
ON THE LEAKAGE OF SUBMARINE CABLES.



A B is a cable, equally leaky throughout. It is 20 miles long. It requires eight Leclanché cells to work a theoretically perfect cable. What insulating power must the dielectric have to produce

the same strength of signal with 12 cells? How many cells will be required to work with an insulation of 100,000 ohms per mile?

The general solution of the problem, of which the above particular cases were submitted to me, is one of much interest, and, although certain cases have been solved more than once, the complete determination of the flow of electricity through a leaky cable is of sufficient practical value to excuse its publication here.

Let the cable A B, of any length, have its further end to earth through a resistance σ. By putting $\sigma = 0$ the end of the cable will be direct to earth, and by putting $\sigma = \infty$, it will be insulated.

Let $\quad AB = n$
$AP = x$
$PQ = dx.$

Let the potential at $\quad A = V_0$
$B = V_n$
$P = V$
$Q = V + dV.$

Let the strength of current at
$A = I$
$B = I_n$
$P = I$
$Q = I + dI.$

Let the resistance of
$XAP = R$
$XAQ = R + dR$
$XAB = R_n$
$XA \quad = R_0 = \sigma.$

Also let resistance of unit length of conductor $= r.$

And let insulation ,, ,, sheathing $= i.$

Calling the electromotive force of the battery E, then since the flow of electricity from any point to any other point close to it is from the point of higher to that of lower potential, and is equal to the difference of potential divided by the resistance separating the two points, therefore the current along A B at P is,

$$\frac{(V + dV) - V}{r\,dx} = \frac{dV}{r\,dx} = I.$$

The resistance of the wire P Q is evidently $r\,dx$, because it varies *directly* as its length, but the resistance of the insulating sheath P Q is $\frac{i}{dx}$, because it varies *inversely* as its length. Hence the "leakage," or the current from the surface of the conductor between the points P and Q to the earth where potential is zero, is

$$\frac{V - 0}{\frac{i}{dx}} = \frac{V\,dx}{i} = d\mathrm{I}.$$

Hence—

$$\frac{d\mathrm{I}}{dx} = \frac{V}{i},$$

but

$$\mathrm{I} = \frac{dV}{r\,dx}$$

and therefore

$$\frac{d\mathrm{I}}{dx} = \frac{1}{r}\,\frac{d^2V}{dx^2},$$

therefore

$$\frac{d^2V}{dx^2} = \frac{rV}{i} = m^2V,$$

where

$$m^2 = \frac{r}{i}.$$

The solution of this differential equation, obtained by the well-known method,* is—

$$V = Ae^{mx} + Be^{-mx} \quad \ldots\ldots\ldots\ldots \quad (1)$$

and

$$\mathrm{I} = \frac{1}{r}\,\frac{dV}{dx} = \frac{m}{r}\left[Ae^{mx} - Be^{-mx}\right] \ldots\ldots \quad (2)$$

Now when $x = n$

$$V = V_n = E \text{ and } \mathrm{I} = \mathrm{I}_n;$$

therefore, since resistance $= \frac{\text{potential}}{\text{intensity}}$

$$R_n = \frac{V_n}{\mathrm{I}_n} = \frac{E}{\mathrm{I}_n}$$

and similarly when $x = 0$

$$V = V_o \text{ and } \mathrm{I} = \mathrm{I}_o$$

and

$$R_o = \sigma = \frac{V_o}{\mathrm{I}_o}.$$

* See "Boole's Differential Equations."

Taking therefore $x = n$

$$E = V_n = Ae^{mn} + Be^{-mn} \qquad \text{by} \quad (1)$$

$$I_n = \frac{m}{r}\left[Ae^{mn} - Be^{-mn}\right] \qquad \text{by} \quad (2)$$

therefore
$$R_n = \frac{E}{I_n} = \frac{r}{m}\left[\frac{Ae^{mn} + Be^{-mn}}{Ae^{mn} - Be^{-mn}}\right] \quad \ldots\ldots \quad (3)$$

Again, taking $x = 0$

$$\sigma = \frac{V_o}{I_o} = \frac{A + B}{\frac{m}{r}(A - B)},$$

therefore
$$\frac{A}{B} = \left(\frac{\sigma\frac{m}{r} + 1}{\sigma\frac{m}{r} - 1}\right) \ldots\ldots\ldots\ldots \quad (4)$$

and
$$R_n = \frac{r}{m}\left[\frac{e^{mn}\left(\sigma\frac{m}{r} + 1\right) + e^{-mn}\left(\sigma\frac{m}{r} - 1\right)}{e^{mn}\left(\sigma\frac{m}{r} + 1\right) - e^{-mn}\left(\sigma\frac{m}{r} - 1\right)}\right] \quad \ldots\ldots \quad (A)$$

Again
$$I_n = \frac{m}{r}\left[Ae^{mn} - Be^{-mn}\right]$$

and
$$I_o = \frac{m}{r}\left[A - B\right];$$

therefore
$$\frac{I_o}{I_n} = \frac{A - B}{Ae^{mn} - Be^{-mn}};$$

but
$$\frac{A}{B} = \left[\frac{\sigma\frac{m}{r} + 1}{\sigma\frac{m}{r} - 1}\right] \text{ by } (4);$$

therefore
$$\frac{I_o}{I_n} = \frac{\dfrac{\sigma\frac{m}{r} + 1}{\sigma\frac{m}{r} - 1} - 1}{\dfrac{\sigma\frac{m}{r} + 1}{\sigma\frac{m}{r} - 1}e^{mn} - e^{-mn}}$$

$$= \frac{2}{e^{mn}\left(\sigma\frac{m}{r} + 1\right) - e^{-mn}\left(\sigma\frac{m}{r} - 1\right)} \quad \ldots\ldots\ldots \quad (B)$$

Also by (A)

$$\mathrm{I}_n = \frac{\mathrm{E}}{\mathrm{R}_n} = \frac{\mathrm{E}m}{r}\left[\frac{e^{mn}\left(\sigma\frac{m}{r}+1\right) - e^{-mn}\left(\sigma\frac{m}{r}-1\right)}{e^{mn}\left(\sigma\frac{m}{r}+1\right) + e^{-mn}\left(\sigma\frac{m}{r}-1\right)}\right] \quad \ldots \text{(C)}$$

and by (B)

$$\mathrm{I}_o = \frac{2\,\mathrm{E}\frac{m}{r}}{e^{mn}\left(\sigma\frac{m}{r}+1\right) + e^{-mn}\left(\sigma\frac{m}{r}-1\right)} \quad \ldots\ldots\ldots \text{(D)}.$$

The formulæ (A), (B), (C), (D), are the formulæ in general required. If it be required to determine r or i, the other elements being given, we must proceed by the method of gradual approximation.

Now let $\sigma = \infty$, *i.e.* let end of cable be insulated, then we have

$$\mathrm{R}_n = \frac{r}{m}\left[\frac{e^{mn}+e^{-mn}}{e^{mn}-e^{-mn}}\right] \quad \ldots\ldots\ldots\ldots (a_1)$$

$$\frac{\mathrm{I}^o}{\mathrm{I}_n} = \frac{2}{\infty} = 0 \quad \ldots\ldots\ldots\ldots (b_1)$$

$$\mathrm{I}_n = \frac{\mathrm{E}m}{r}\left[\frac{e^{mn}-e^{-mn}}{e^{mn}+e^{-mn}}\right] \quad \ldots\ldots\ldots\ldots (c_1)$$

$$\mathrm{I}_o = 0 \quad \ldots\ldots\ldots\ldots (d_1).$$

Next let $\sigma = 0$, *i.e.* let end of cable be put to earth, then

$$\mathrm{R}_n = \frac{r}{m}\left[\frac{e^{mn}-e^{-mn}}{e^{mn}+e^{-mn}}\right] \quad \ldots\ldots\ldots\ldots (a_2)$$

$$\frac{\mathrm{I}_o}{\mathrm{I}_n} = \frac{2}{e^{mn}+e^{-mn}} = \frac{2}{e^{mn}+\frac{1}{e^{mn}}} \quad \ldots\ldots\ldots\ldots (b_2)$$

$$\mathrm{I}_n = \frac{\mathrm{E}m}{r}\left[\frac{e^{mn}+e^{-mn}}{e^{mn}-e^{-mn}}\right] \quad \ldots\ldots\ldots\ldots (c_2)$$

$$\mathrm{I}_o = \frac{2\,\mathrm{E}\frac{m}{r}}{e^{mn}-e^{-mn}} \quad \ldots\ldots\ldots\ldots (d_2).$$

I may point out, with reference to formula (b_2), that it agrees with that given by Professor Fleeming Jenkin in his *Electricity and Magnetism*, page 345, where, however, by an obvious misprint, e^{mn} has been given as $e\,m\,n$ (the m in his treatise corresponds with the i in the above formula).

Again, having had given the electro-motive force E^1 necessary to produce a signal of a certain strength through a perfect cable, to find what electro-motive force E will be necessary to produce the same strength of current through a uniformly leaky cable.

For this we have by (d_2)

$$I_o = \frac{2E\frac{m}{r}}{e^{mn} - e^{-mn}}$$

and
$$I_o = \frac{E^1}{nr};$$

therefore—
$$E = \frac{E^1 (e^{mn} - e^{-mn})}{2\frac{m}{r} \cdot nr}$$

$$= \frac{E^1 (e^{mn} - e^{-mn})}{2mn}.$$

It may be as well to notice, that from equations (a_1) and (a_2) the product of the resistance of the cable when its end is to earth, and its resistance when its end is insulated, equals—

$$\frac{r^2}{m^2} = \frac{r^2}{\frac{r}{i}} = ri$$

i.e. is constant.

We now proceed to apply the general results obtained to the cases propounded and others.

Examples:—

The cable we will suppose to have a conductor resistance of 10 ohms per mile.

The length of the cable is 20 miles.

First—

To find the total resistance of the cable when to earth at the end.

If perfect the resistance will be $20 \times 10 = \underline{200}$ ohms.

Suppose now the insulation resistance of each mile of cable to be 100,000 ohms then—

$$m = \sqrt{\frac{10}{100000}} = \frac{1}{100}$$

$$mn = \frac{1}{100} \times 20 = \frac{1}{5}$$

$$\text{Total Res.} = 1000\left(\frac{2{\cdot}7183^{\frac{1}{5}} - 2{\cdot}7183^{-\frac{1}{5}}}{2{\cdot}7183^{\frac{1}{5}} + 2{\cdot}7183^{-\frac{1}{5}}}\right)$$

$$= \underline{197{\cdot}38} \text{ ohms.}$$

Second—

To find the total resistance of the cable when the end is insulated.

If perfect the resistance will be $= \infty$.

If not

$$\text{Total Res.} = 1000\left(\frac{2{\cdot}7183^{\frac{1}{5}} + 2{\cdot}7138^{-\frac{1}{5}}}{2{\cdot}7183^{\frac{1}{5}} - 2{\cdot}1783^{-\frac{1}{5}}}\right)$$

$$= \underline{5066{\cdot}4} \text{ ohms.}$$

The result which would be obtained by the common process would have been

$$\frac{100000}{20} = \underline{5000{\cdot}0} \text{ ohms.}$$

Third—

Having given the electromotive force E, what will be the strength of the current flowing out and the current flowing in?

Let $E = 12$,

$$\text{Current flowing out} = I_o = \frac{2 \times \frac{1}{100} \times 12}{10} \cdot \frac{1}{2{\cdot}7183^{\frac{1}{5}} - 2{\cdot}7183^{-\frac{1}{5}}}$$

$$= \underline{{\cdot}059601}.$$

$$\text{Current flowing in} = I_n = \frac{E}{\text{Res.}} = \frac{12}{197{\cdot}38} = \underline{{\cdot}060796}.$$

Fourth—

Having given the electromotive force E^1 necessary to produce a signal of a certain strength through a perfect cable, to find what electromotive force E will be necessary to produce the same strength of current through a uniformly leaky cable.

$$E = \frac{E^1}{2mn}\left(e^{mn} - e^{-mn}\right)$$

$$= \frac{8}{2 \times \frac{1}{100} \times 20}\left(2{\cdot}7183^{\frac{1}{5}} - 2{\cdot}7183^{-\frac{1}{5}}\right)$$

$$= \underline{8{\cdot}0535} \text{ cells.}$$

Fifth—

Having given the electromotive force E^1 necessary to work a perfect cable, what insulating power must the dielectric have to produce the same strength of signal with an electromotive force E?

Having given $E = 12$, $E^1 = 8$, to find what i equals.

This must be done by gradual approximation.

The value will be found to be

$$i = \underline{1520} \text{ ohms.}$$

I propose in a subsequent paper to deal with the case of an overland wire supported on imperfectly insulated poles, so that there is an escape at a series of detached points.

A. B. KEMPE, B.A.

A NEW METHOD FOR THE DETERMINATION OF THE DISTANCE OF A FAULT IN SUBMARINE CABLES.

The methods hitherto employed are not very precise, especially when the cable not being broken the fault is such as not to prevent communications between extreme stations. The polarisation produced at the fault vitiates also the results obtained, and it is not possible to take in account the influence of natural currents circulating in the cable. Those who have had to determine a fault of this nature know all these difficulties.

The method herewith introduced is a combination of that published by the author, *On the Determination of Voltaic Constants* (see *Journal Télégraphique of Berne*, 25th Jan. 1873), and of the proceedings indicated by Latimer Clark in his paper *On a Voltaic Standard of Electromotive Force* (see *Proceedings of the Royal Society*, vol. xx. p. 444, 1872).

The influence of polarisation at the fault is entirely avoided, as well as that due to the presence of natural currents. The distance to the fault is obtained without making any comparison with previous tests, and there is no correction to be made for the temperature of the cable, and therefore its conductivity at the time

of the experiments. This method is likewise independent of variations which may take place in the electromotive force or resistance of the batteries employed.

It therefore seems to combine conditions of accuracy far surpassing those that can be obtained by other methods in the same case indicated, which is, however, one of the most difficult.

We shall first explain this method theoretically, then we shall examine the means to be employed for its practical application. It is sufficient, for the present, to observe that the experiments are made simultaneously at each end of the cable.

A B is the cable, and C the fault.

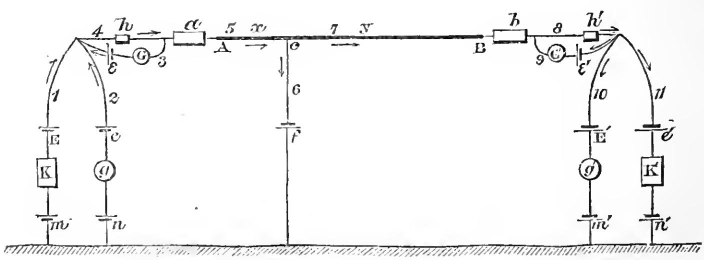
K, h, a and K′, h', b, are resistances. K and K′ being variable, and arbitrarily graduated, or even not graduated at all—h and h' are adjusted so as to allow a determined and previously fixed resistance, or its double, to be taken at will. a and b are two slide resistance-boxes, graduated in the same units as h and h'.

G, g, and G′, g', are Thomson's galvanometers.

Σ and Σ' are batteries formed, each of them, with one standard cell of Latimer Clark's pattern, and therefore equal. e and e' are also batteries formed with elements of Latimer Clark's type.

E and E′ are batteries of any kind, but having a total electromotive force superior to that of e and e'. They are, therefore, the only ones producing current and work.

Finally, m, n, and m', n', represent the natural electromotive forces which may develope themselves in and act on the cable.



The batteries being arranged, as shown by above diagram, viz., in such a way that currents leaving e and e' add themselves in the

cable, and are opposed to the corresponding currents starting from E and E′; and from Σ and Σ′, we have, according to Kirchoff's law, the following relations, naming i the currents and r the resistances, corresponding to each part of the circuit:

$$i_1 + i_2 + i_3 = i_4 \qquad i_2 r_2 + i_4 r_4 + i_5 r_5 + i_6 r_6 = n + e + f$$
$$i_3 + i_5 = i_4 \qquad i_3 r_3 + i_4 r_4 = \Sigma$$
$$i_6 + i_7 = i_5$$
$$i_7 + i_9 = i_8 \qquad i_{10} r_{10} + i_8 r_8 + i_7 r_7 - i_6 r_6 = n' + e' - f$$
$$i_9 + i_{10} + i_{11} = i_8 \qquad i_8 r_8 + i_9 r_9 = \Sigma'$$

In order that no polarisation should take place in b, the current there should be nil and $i_6 = o$,
wherefrom $i_5 = i_7$.

To avoid polarisation we must therefore arrange so that there are equal currents in A and B. Σ and Σ′ should therefore be made equal, which is already the case, and if we introduce in h and h' two resistances conveniently selected, modifying at the same time a and b so as to bring to zero the needles of galvanometers G and G′, we shall have

$$i_3 = o \qquad \text{and} \qquad i_9 = o.$$

Moreover we may eliminate the values of resistances in batteries E and E′, thus doing away at the same time with the effect of variability in their electromotive forces, by modifying K and K′ so as to bring also to zero the needles of galvanometers g and g'. We have thus—

$$i_2 = o \qquad \text{and} \qquad i_{10} - o.$$

So that all the preceeding relations become

$$i_1 = i_4 = i_5 = i_7 = i_8 = i_{11} = \mathrm{I}$$
$$\mathrm{I}\,(r_4 + r_5) = n + e + f \qquad \mathrm{I}\,(r_8 + r_7) = n' + e' - f$$
$$\mathrm{I}\,.\,r_4 = \Sigma \qquad \mathrm{I}\,.\,r_8 = \Sigma$$

To obtain equal currents we must therefore make $r_4 = r_8$, that is to say, we must have $h = h'$ as it is evident that $r_4 = h$. From the above equations we obtain—

$$\frac{n + e + f}{h + r_5} = \mathrm{I} \qquad \text{and} \qquad \frac{n' + e' - f}{h + r_7} = \mathrm{I}.$$

After having realised these conditions, the same operation is repeated by reducing the circulating current to half its value. It

is sufficient in that case to double h and to vary the resistance a and b until the galvanometers return to zero. Then we have again—

$$\frac{n+e+f}{2h+R_5}=\frac{I}{2} \quad \text{and} \quad \frac{n'+e'-f}{2h+R_7}=\frac{I}{2}$$

From these four relations we obtain the two following—

$$2r_5 = R_5 \quad \text{and} \quad 2r_7 = R_7$$

we have on the other hand

$$r_5 = a + x \qquad r_7 = b + y$$
$$\text{and}$$
$$R_5 = A + y \qquad R_7 = B + y$$

A and B being the new values of resistances in a and b, therefore

$$x = A - 2a \quad \text{and} \quad y = B - 2b.$$

If we operate so as to make the experiments in quick succession, thus avoiding the variations which might arise in the natural electromotive forces n, n', and f, the result obtained will be altogether independent of the acting batteries, of polarization at the fault and of the presence of natural currents.

Let l express the length of the cable and d the distance to the fault on side A C, we have evidently $\frac{l-d}{d}=\frac{y}{x}$ wherefrom we find for the required distance

$$d=\frac{l\,(A-2a)}{A+B-2\,(a+b)}.$$

Practical Operation.

The series of manipulation described above may appear at first tedious and difficult, but it is not so, especially if the distance to the fault is already known approximately, and if we make $e = e'$, as in that case we may determine very closely the values to give to resistances a and b in both successive experiments, and then we shall easily and quickly bring the needles of both galvanometers G and G′ to zero.

At each end of the cable there should be an electrician and his assistant. The latter will manipulate resistance k by sliding it gently either right or left until the needle of galvanometer g comes to zero.

The electrician acts on resistance a, after having taken for h a convenient quantity previously determined. He thus tries to maintain the needle of galvanometer G at zero.

To proceed with experiments, when everything has been previously concerted, when h is determined and g is at zero, send both currents, then galvanometers G and G′ deviate, and resistances a and b have to be gently altered until the two galvanometers return to zero.

Suppose that the electrician placed at G be in charge of the whole operation, as long as his galvanometer stands at zero he knows that G′ is also at zero, and therefore that resistance b remains unaltered.

This done, he alters h to $2h$, which moves the needles of both galvanometers and serves as a signal for G′, who also alters h' into $2h'$, and on each side galvanometers are brought to zero in the same way as before.

After these experiments the numbers found on the rheostats at the nearest time of the alteration of h in $2h$ are exchanged between both electricians.

These operations can be repeated several times in order to obtain mean results.

If it is desired to find the resistance of the fault, it will be easy, knowing its distance, to determine it by one of the known methods.

EMILE LACOINE.

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ABSTRACTS AND EXTRACTS.

PHIL. MAG. *Vol. XL. June, No.* 327.

ON THE ELECTRIC CONDUCTING POWER OF THE CHLORIDES OF THE ALKALIES, ALKALINE EARTHS, AND NITRIC ACID IN AQUEOUS SOLUTIONS.

By F. KOHLRAUSCH and O. GROTRIAN.

Experiments were made on thirty-five different solutions of the chlorides of the alkalies and alkaline earths, which showed the dependence of the conducting power on the amount of salt contained, and on the temperature from 0° to 40° C.; seven solutions of nitric acid were also experimented on.

Certain precautions were taken so as to avoid error due to the polarisation of the electrodes between which the liquids were placed.

The observations of each solution were comprehended in the formula,

$$K_t = K_o (1 + at + \beta t^2)$$

in which K_t signifies the conductivity at the temperature *t*. The result of the experiments proved that:—

The conducting power increases in nearly equal proportion with the temperature.

The temperature co-efficients for the different chlorides in dilute solutions were approximately equal.

With increasing amount of salt contained all the temperature co-efficients at first diminish. Afterwards it was found that KCl, NH_4Cl, and $BaCl_2$ show a diminution of the co-efficients up to the greatest concentration, whilst $NaCl$, $CaCl_2$, and $MgCl_2$, on the contrary, have a minimum between 10 and 20 per cent.

The experiments tended to advance a theory "that equal volumes of anhydrous salts in solutions imply equal conducting power."

The best conducting salt was found to be the ammonic chloride, which conducted about one-half as well as the best conducting acids known,

when 25 per cent. of the salt was present in the solution. At a temperature of 100° it conducted quite as well as the acid solution.

It is pointed out from this that the sal-ammoniac solution could be employed with advantage in voltaic batteries in preference to the strongest acids.

THE ELECTROLYSIS OF CERTAIN METALLIC CHLORIDES.

By J. H. Gladstone, F.R.S., and Alfred Tribe, F.C.S.

In previous experiments it had been shown that nitrate of copper brought into tension by silver and copper in conjunction is decomposed by free oxygen in solution. It was now endeavoured to substitute chlorine in the form of chloride of copper in the place of oxygen.

When a copper and platinum plate are immersed in cupric chloride, cupreous chloride forms on the copper plate alone, but if the two plates are connected by a wire it is found that both plates become coated. This deposit on the platinum plate was only strongly marked when the solutions contained from 2·5 to 10 per cent. of the salt. A 40 per cent. solution gave no deposit; the copper plate was, however, coated as before.

When platinum electrodes in a solution of the cupric chloride were excited by a single zinc-platinum cell, excited by common water, cupreous chlorine was deposited on the negative electrode and chloride liberated at the positive. A Grove's cell first deposited cupreous chloride and then metallic copper at the negative pole, chlorine being formed as before at the positive plate.

Substituting a zinc plate in the place of the copper one in the first experiment, and joining their poles, a more energetic action was produced, metallic copper being deposited on the platinum plate besides the cupreous chloride.

The actions were tried with other chlorides and the same results were obtained.

ON THE FLOW OF ELECTRICITY IN A UNIFORM PLANE CONDUCTING SURFACE.

By G. Carey Foster, F.R.S., and Oliver J. Lodge.

The first case considered is that of one pole in an infinite sheet. The lines of flow being evidently in this case straight lines radiating from the pole. The resistance of a portion of the sheet bounded by two circles of which the pole is the centre is shown to be

$$R = \frac{1}{2\pi \kappa \delta} \cdot \log \frac{r_2}{r_1},$$

in which r_2 is the radius of the outer circle, r_1 that of the inner, δ the thickness of the plate, and κ a constant; the equation being in fact the one for the resistance of the core of a submarine cable in which r_2 is the outer diameter or radius, r_1 the diameter or radius of the conductor, and δ the length.

The next proposition taken was that of two equal and opposite poles in an infinite sheet, the two poles of a battery in fact.

The lines of flow in this case are shown to be circles, but not consecutive ones, passing through the poles, each one differing from its neighbour by a constant change.

The lines of equipotential, that is, lines in which if we placed the two wires from a galvanometer we should obtain no deflection, were shown to be circles all of which were centered in a straight line passing through the two poles, and whose circumferences cut the lines of flow at right angles.

If r_1 and r_2 are the distances of these circles from one pole, and $r_1{}^1$ and $r_2{}^1$ the distances from the other pole, then the resistance between the poles is expressed by the formula

$$R = \frac{1}{2\pi \kappa \delta} \cdot \log \frac{r_1{}^1 r_2}{r_1 r_2{}^1}$$

or taking ρ_1 the radius of the circle at the positive pole or source, ρ_2 the radius of the circle at the negative pole or sink, and $2a$ the distance between the poles, then

$$R = \frac{1}{2 \quad \kappa \delta} \cdot \log \frac{\rho_2}{\rho_1}$$

When the circles are over the same pole the case is the same as for

1 pole on an infinite sheet, the difference being that the circles in this latter case are not concentric.

When the circles surround opposite poles, if the radii are equal, then—

$$R = \frac{1}{\pi \kappa \delta} \cdot \log. \frac{a + \sqrt{a^2 + \rho^2}}{\rho}$$

or when the common radius is small as compared with a, or as we may take it the diameter of the wires in the sheet, then—

$$R = \frac{1}{\pi \kappa \delta} \cdot \log. \frac{\rho\, a}{\rho}$$

The resistance of the wires at the edge of a disc and on opposite sides is given by the formula

$$R = \frac{2}{\pi \kappa \delta} \quad \log. \frac{D}{\rho}$$

where D is the diameter of the disc.

A general result for any number of poles in an infinite sheet is also discussed.

(*Vol. XL., July, No.* 328.)

AN EXPERIMENT FOR SHOWING THE ELECTRIC CONDUCTIVITY OF VARIOUS FORMS OF CARBON.

By H. Bannerman, F.C.S.

A fragment of the substance to be tested is held between the jaws of a pair of tongues, formed by bending a strip of sheet-zinc into a horseshoe form, and immersed in a solution of cupric sulphate. If the carbon is a non-conductor the copper salt is decomposed, and a deposit of copper takes place on the zinc alone, but if the carbon conducts, the deposit takes place on the carbon, in consequence of the zinc and carbon forming a galvanic couple.

A comparative conductivity would thus be roughly shown by the rate at which the deposit takes place on the various kinds of carbon experimented on.

It was found that American anthracite and coals, which had been subjected to the action of intruded igneous rocks, conducted best.

Some Peruvian anthracite, containing a large amount of sulphur, conducted almost as well as graphite. Welsh anthracite did not appear to conduct until after it had been heated red-hot, when it conducted freely.

It is suggested that these experiments might give a possible clue to the temperature at which anthracitic metamorphism of coals has been effected in different districts.

CENTRAL TELEGRAPH STATIONS.

The great commercial capitals of the world, London and New York, have within a very short period been provided with Central Telegraph Offices which for extent and completeness are unequalled elsewhere. The Central Telegraph Station in St. Martin's-le-Grand, and the new offices of the Western Union Telegraph Company in Broadway, New York, may be regarded as evidences of an amount of progress such as has attended few institutions in our time. Less than thirty years ago the "system" of the late Electric Telegraph Company, or, to speak more correctly, the telegraphic system of the United Kingdom, consisted of a line to Nine Elms, and a small office at 334, Strand. Similarly, in 1846, a single wire was erected to an obscure office beneath the express offices at No. 16, Wall Street, New York, and two wires from Washington terminated in a small room over the Ferry-house in Jersey City, where three clerks easily, and not very continuously, performed the whole telegraphic business of the city of New York. We need not trace the progress of the Electric Telegraph Company eastward until it acquired extensive offices, first, in Founders' Court, Lothbury, and, subsequently, in Telegraph Street, Moorgate Street. Nor need we do more than simply mention the British and Irish Magnetic Telegraph Company, with its offices in Threadneedle Street, and the United Kingdom Telegraph Company, located in Gresham Street. All three were eventually housed in the premises built by the Electric Company in Telegraph Street, and thence the next move was to the new Post-Office in St. Martin's-le-Grand. This event occurred on the 17th of January, 1874; and, little more than a year afterwards—viz., on the 1st of February last—the Great American Telegraph Company moved to its new premises in Broadway, New York.

The American structure has been erected at a cost of more than two million dollars, and a considerable portion of this amount has been subscribed in England—chiefly in London, we believe. It is built of

brick and granite, in what, with some latitude, may be designated as the French *renaissance* style, the main idea in its construction being to reduce in appearance, by the proportions and the arrangement of the details, the great height of the building, as compared with its width or front. The building is said to be fire-proof throughout, wood having only having been used for the doors, window sashes, and the wainscoting. Most people are familiar with the appearance of our new Post-Office buildings in St. Martin's-le-Grand. Many regard them as being wholly devoted to telegraph purposes; but it should be explained that only the top floor, the basement, and one or two rooms on the intervening floors are so occupied. The building of the Western Union Telegraph Company in New York extends to the height of no fewer than ten separate floors, and is mainly occupied by the various offices of the Company; but it is with the operating rooms, situated on the seventh floor, just as it is with the instrument galleries in St. Martin's-le-Grand, that we are mainly concerned in speaking of both as central telegraph stations. The American room is 145 feet long, 70 feet wide, and 23 feet high, or about the size of the central gallery in St. Martin's-le-Grand. This gallery, supplemented by the side wings, and forming a space somewhat resembling the letter H, has a superficial area of not less than 20,000 feet; so that the American room is not more than half the size of our own. The instrument tables do not extend to more than 500 feet in length in New York; while those at St. Martin's-le-Grand extend to 2,800 feet, or more than half a mile. In America, where the system of "sound" telegraphy prevails to a large extent, the tables are cut into short lengths, each separated into four compartments, so as to isolate the operators from each other and to confine the sound as much as possible. Here, owing to the variety of systems employed and the extensive use of automatic instruments, the tables are of considerable length, and are open throughout. In the Western Union Company's new office less than 200 instruments of all kinds are employed, including 149 Morse instruments, fifteen sets of Duplex apparatus, and six of Phelps's printing instruments. In St. Martin's-le-Grand the total number of instruments exceeds 450, and includes 195 Morse printers, 122 single-needle instruments, 65 sets of Duplex apparatus, 53 sets of Wheatstone's automatic instruments, and 18 of other sorts. The switch or test-board of the American office is arranged for the distribution of 300 wires; that in our own Post-Office building is arranged to accommodate 800 circuits, and if need be

the provision can be extended to 1,000 lines without difficulty. Batteries, which are to the telegraph what the boiler is to the locomotive, are always an object of anxiety in planning a large telegraph office. They cannot well be situated in the instrument room on account of their peculiar construction and their continual wants; and yet they should not be too far away from it. In this respect the New York office, where the batteries are stored in a room immediately underneath the operating room, has a decided advantage over the London office, where several floors intervene between the instrument galleries and the battery room. In extent, however, the two departments are as widely different as are the arrangements in the respective instrument rooms. In the Western Union Company's office provision is only made for a *maximum* power of less than 17,000 cells; while the number of cells actually in use does not exceed 7,000. At St. Martin's-le-Grand 50,000 cells can readily be accommodated in the large room in the basement of the building set apart as a battery store; while at the present moment not fewer than 23,000 cells are actually in use. Not far short of two miles of shelving have been constructed in this room for the reception of batteries, and a perfect avalanche of wires descends from the instrument galleries above in order to transmit the motive power to the 450 instruments, whose wants are as numerous as they are varied and unceasing. Including these battery connections and other connections between different points in the instrument galleries, not less than 260 miles of gutta-percha covered wire are buried under the floors of the building in St. Martin's-le-Grand.

We have been struck with the small extent of the pneumatic system in the great telegraph office of New York. Apparently the system is confined to the building itself, and does not extend beyond the receiving and delivery departments. A single 20-horse power engine is all the motive power required in connection with this department, the remaining machinery, situated in the basement of the building, being required in connection with the elevators, and for heating purposes. The pneumatic system at St. Martin's-le-Grand is one of the great features of the building. No fewer than twenty-five separate tubes communicate with out-stations in the Metropolitan district, ranging from Fenchurch Street to Tower Hill in the east, to Temple Bar and Charing Cross in the west. These tubes extend to a length of nearly eighteen miles, and are worked outwards by pressure and inwards by vacuum. In addition to these

outlying tubes there are twelve tubes within the building itself, used for blowing messages between one part of the instrument gallery and another. So rapidly is this effected that an average of four seconds only is occupied by the "carrier" in making the journey across the room, or from one wing to another, as the case may be. The motive power by which the tubes are worked exists in the basement of the building, in the shape of three steam engines, each of 50-horse power. Two of these are constantly employed in pumping air into or sucking it out of huge mains carried up the outer walls of the building, and connected with the tubes up-stairs. The third is at rest, ready for any emergency, or to take the place of that whose turn for rest next comes round. The engine-room resembles nothing so much as the hold of a great steamship; and from the peculiarly interesting character of the machinery it is a great source of attraction to the numerous visitors to the Central Telegraph Office. Four boilers, each of 50-horse power, and fitted with Vigars' patent stokers, occupy a corresponding position of the basement to that occupied by the engines; and an Artesian well is in process of sinking, which, it is hoped, will supply not only the boilers, but the whole of the building with water.

It only remains to notice the *personnel* of the two great Telegraph offices of the world, and to sum up the amount of business transacted in each. Less than 300 persons, including 75 female clerks, are employed by the Western Union Company in its central office in New York, and the average number of messages disposed of daily, exclusive of news messages, is stated to be 24,000. Allowing for the news service, which is stated to amount to about 90,000 words daily, an average of 27,000 messages a day would be attained. Here, in London, as many as 1,200 persons, including 700 females, are employed in the Central Telegraph Station. The number of ordinary commercial messages dealt with in a day, allowing for such as have to be retransmitted—*i.e.*, received on one wire and sent out on another—is upwards of 50,000, and taking the news service, exclusive of special wires, at 500,000 words a day, which is a fair average, during the Parliamentary Session a total of more than 70,000 average messages would be reached. The news service is a feature to which very special importance is attached at our Central Telegraph Station. Special wires, known as the "Express Circuits," are set apart for the service, and the Wheatstone system, which, while it economises the wires, largely increases the staff, is almost exclusively

employed. At night, when the great bulk of the news-work comes in, as many as 40 wires are exclusively occupied in the transmission of matter for the press throughout the United Kingdom. Nineteen of these wires are leased out to certain provincial newspapers in England, Scotland, and Ireland. The remaining 21 are worked to the principal towns of the kingdom, and taking their united capacity, it would be found that on a fairly busy night as many as half a million of words, equal to 250 closely printed columns of *The Times*, would be disposed of. Besides the clerks employed at the newspaper offices in working the special wires, as many as 200 clerks are employed each night at the Central Telegraph Station between 8 P.M., when the female staff leaves duty, and midnight when the bulk of the news work has generally been disposed of. From midnight until 2 A.M., more than 100 clerks are usually employed; and at no time of the night is the number less than 70. Scarcely, indeed, has the night service of news been completed when the morning service begins, and it would be almost impossible to select any hour out of the 24 when press matter, either in the shape of markets, exchanges, or general news, Reuter, sporting, or Parliamentary, is not passing over the wires.

If the telegraphic system is not yet perfect, it will be granted, at least, that its development thus far has been singularly rapid, and there is, perhaps, no better evidence of this than in the great central offices of London and New York. After all, these immense establishments are but the growth of little more than a quarter of a century; for, as we have seen, it is less than 30 years since a small room in West Strand, and another in Wall Street, represented the head quarters of the telegraph system of the Old World and the New.—*Times*.

ELECTRIC TELEGRAPHY IN AUSTRALIA.

A very interesting lecture or *conversazione* was given last evening in the Athenæum Hall by the members of the Victorian Electrical Society. There was a very large attendance, and the chair was occupied by Mr. Turner, Deputy Postmaster-General, who made a few introductory remarks. The Society had provided plenty of instruments for experiments, and to Mr. Daniels was entrusted the task of explaining the whole of the proceedings, which he did in a very lucid manner. During the evening the systems of Morse and Wheatstone were fully explained, and in order to elucidate the manner in which telegraphic messages were transmitted from one station to another, a station was arranged on the platform, and another in the gallery, with an intermediate station halfway down the hall. At the former Messrs. M'Gauran and Smibert officiated; in the gallery Messrs. Clay and Jenvey were stationed, while Mr. Cumming was the operator at the intermediate station. The first part of the evening's entertainment was devoted to the explanation, in as simple a manner as possible, of the various systems of working the electric telegraph, and this was very lucidly done by Mr. Daniels, as was also the system of the construction of the circuit, so that the messages might be transmitted on the various lines. He explained that under the present system it was a matter of impossibility to send a message from both ends of the lines at the same time, and when two messages crossed on the way, it became necessary for the operators to break the circuit, and allow one message to pass along the line before the other message was transmitted. The rate of speed attainable in sending messages by the Morse system was about the same as that attainable by a fast writer; but he thought there were not many writers with the pen who could keep up with some of the more expert operators. Twenty-five words per minute was the presumed competent operator's limit, but as much beyond that as he could do was his ambition, and as much as eighty words per minute, under certain conditions, had been attained. At the last Melbourne Cup meeting one operator sent two hundred and twenty messages in two hours—a feat only surpassed by the skill of the operator who received them. This part of the proceedings was illustrated by the transmission and reception of messages from different parts of the hall. Mr. Daniels then proceeded to give a scientific explanation of the system of duplex telegraphy, or the

sending of two messages in opposite directions at the same time by the same wire. He stated that the system was not at all new, as it was discovered in 1853, but had not hitherto come into general operation. After making the explanation, Mr. Daniels had to admit that the explanation was difficult to understand without fully understanding the full working of the whole of the telegraph system. That the duplex system could be successfully worked was shown by the fact that several messages were sent from the platform and the gallery, and *vice versâ*. The messages crossed one another most successfully, and were received simultaneously at each end of the hall. One of the questions asked was, whether messages could be transmitted without any wire, and Mr. Daniels answered this in the affirmative by stating that it could be done at short distances, and by means of a new American invention called a "snapper sounder," which can be carried in the waistcoat pocket, transmitted a message from the platform to the gallery. The remainder of the entertainment consisted of a number of interesting experiments with the electrical machine, including the explosion of torpedoes by electricity, all of which were attentively watched by the audience, and loudly applauded. It had been intended to give an explanation of the drum mystery, as shown by Heller and Hasalmayer at their entertainments, but owing to some defect in the battery the circuit could not be obtained, and as the instrument would not work, Mr. Daniels apologised to the audience for the excision from the programme. A vote of thanks to the chairman terminated the proceedings.—*The* (Melbourne, Australia) *Argus*, February 2nd.

ELECTRICAL RESISTANCE OF VARIOUS METALS.

M. Benoit has measured with great precision the electrical resistance of various metals at temperatures from 0° to 860°. He employed both the method of the differential galvanometer and of the Wheatstone's bridge, and for each method has measured several specimens. The mean of these is given in the following table, the second column giving the resistance of a wire, 39·37 inches long and having a cross section of 0·03

inches in ohms, and column three the same quantity in Siemens' units. Column four gives the resistance compared with silver:—

Metal.	Ohms.	Siemens.	
Silver, A.	·0154	·0161	100
Copper A.	·0171	·0179	90
Silver, A. (1)	·0193	·0201	80
Gold A.	·0217	·0227	71
Aluminum, A. . . .	·0309	·0324	49·7
Magnesium, H. . . .	·0423	·0443	36·4
Zinc, A. at 350°. . .	·0565	·0591	27·5
Zinc, H.	·0594	·0621	25·9
Cadmium, H	·0685	·0716	22·5
Brass, A. (2)	·0691	·0723	22·3
Steel, A.	·1099	·1149	14
Tin	·1161	·1214	13·3
Aluminum bronze, A (3)	·1189	·1243	13
Iron, A.	·1216	·1272	12·7
Palladium, A. . . .	·1384	·1447	11·1
Platinum, A. . . .	·1575	·1647	9·77
Thallium	·1831	·1914	8·41
Lead	·1985	·2075	77·60
German silver, A. (4) .	·2654	·2775	5·80
Mercury	·9564	1·0000	1·61

A. annealed; H. hardened; (1) silver ·75; (2) copper 64·2, zinc 33·1, lead 0·4; (3) copper 90, aluminum 10; (4) copper 50, nickel 25, zinc 25.

These results, which are all taken at ·0, agree closely with those obtained by other observers. M. Benoit has extended his observations to a range of temperature much greater than those previously employed for this purpose. He wound the wire around a clay pipe inclosed in a muffle, and immersed the whole in a bath of water, mercury, sulphur, or cadmium, which was kept at a boiling point by a Perret furnace. Constant temperatures of 100°, 360°, 440°, and 860° were thus obtained. Various temperatures below 360° were also obtained by a mercury bath. The measures were also corrected for expansion. Plates annexed to this memoir, presented to the Faculty of Science of Paris, show the results graphically. They show that the resistance increases regularly for all metals like tin, lead, and zinc up to their points of fusion. This increase, however, differs for different metals. We notice that tin, thallium, cadmium, zinc, lead, are found together in the upper part of the plate; at

200° to 230° their resistance has doubled. Below them are iron and steel; for the last the resistance doubles at 180°, quadruples at 430°, and at 860° is about nine times that at 0°. Palladium and platinum, on the other hand, increase much less, and only double their resistance at 400° to 450°. Gold, copper, and silver form an intermediate group. In general the conductibility decreases more rapidly in a metal the lower its point of fusion. Iron and steel are exceptions to this rule. In alloys the variation is always less than in their constituents, and this is especially the case with German silver.—*Silliman's Journal.*

MECHANICAL AND ELECTRICAL TESTS OF AMERICAN IRON WIRE.

The following table contains a summary of the results of a series of mechanical and electrical tests recently made upon four samples of galvanised wire of American manufacture, the sizes being among those commonly used in telegraphic construction.

	Mechanical.				Electrical.	
Sample Mark.	Weight per Mile (lbs).	Breaking Strain (lbs).	Per Cent. of Elongation.	No. of Twists) 6 in.)	Per Cent. Conductivity Pure Copper=100.	Resistance per Mile Ohms at 60 deg. Fah.
151	282·8	780, 760 } 770	10	25, 28 } 26·5	21·9	16·1
146	287·5	825, 840 } 832·5	16	37, 31 } 29·	21·6	16·1
A H	293·5	1260, 1255 } 1257·5	16	28, 27 } 27·5	15·1	22·7
443	378·1	1640, 1630 } 1635	10	29, 33 } 31	16·5	16·1

The above results seem to point to one very interesting as well as important fact, viz., the close relation existing between the tensile strength and the electrical resistance of iron wire. It will be observed that the first three samples tested are of nearly the same gauge or weight per mile, the size being that usually designated as No. 9½. The tensile strength or breaking strain of the third sample is some 50 per cent. greater than that of the first two, while its specific electrical resistance is also comparatively very high. The proportionate tensile strength of the last two samples is very nearly equal, and so also is their proportionate conductivity, as compared with pure copper, as shown in the sixth column of the table. There seems to be no apparent relation existing between the conductivity or tensile strength of the several wires, and the percentage of elongation, or the number of twists that a given length will sustain before breaking.

The high conductivity of the first two samples is very remarkable. The conductivity of iron is generally assumed by the best authorities to be one-seventh that of pure copper, or about the same as that of the third sample, but it will be observed that the first two samples have a conductivity averaging nearly 22 per cent. that of pure copper. Thus the first samples (No. 9½), weighing but 282·8 lbs. per mile, actually has as much conducting power, mile for mile, as the fourth sample (No. 8), weighing 378·1 lbs. per mile.

The following table includes the first, and gives the result of several other samples of wire :—

Sample Mark and Gauge.	Mechanical.					Electrical,	
	Weight per mile (lbs.)	Per Cent. of Elongation.	No. of Twists (6 in.)	Actual Breaking Strain (lbs.)	Relative Breaking Strain.	Per Cent. Conductivity Pure Copper = 100.	Resistance per Mile in Ohms at 60° Fah.
EBB. Galv. No. 12	190·83	11·5	14, 16 } 15	430, 405 } 417·5	11552·2	14·4	30·5
EBB. Galv. No. 8	381·66	17·7	24, 29 } 26·5	945, 930 } 937·5	12930·6	17·3	12·67
EBB. Galv. No. 11	222·64	17·2	21, 22 } 21·5	575, 580 } 577·5	13639·4	15·6	24·2
151. No. 9½	282·8	10	25, 28 } 26·5	760, 780 } 770·	14275·9	21·9	16·1
EBB. Galv. No. 10	254·44	17·7	28, 29 } 28·5	675, 720 } 697·5	14478·1	17·8	18·42
146. No. 9½	287·5	16	27, 31 } 29	825, 840 } 832·5	15288·86	21·9	16·1
EBB. Galv. No. 6	508·88	11·4	21, 22 } 21·5	1585, 1590 } 1587·5	16462·4	17·7	9·21
EBB. Galv. No. 9	318·05	19·3	17, 18 } 17·5	1005, 1010 } 1007·5	16725·1	16·9	15·54
Nashua ,, No. 8	381·66	15·1	25, 28 } 26·5	1530, 1540 } 1535	21183·	14·7	15
MS. Plain No. 6	528	10·4	18, 21 } 19·5	2110, 2165 } 2137·5	21375·	13·5	11·78
443. No. 8	378·1	10	29, 33 } 31	1630, 1640 } 1635	22301·4	16·5	16·1
A H. No 9½	293·5	16	27, 28 } 27·5	1255, 1260 } 1257·5	22635·	15·1	22·7

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The Thirty-third Ordinary General Meeting was held on Wednesday, the 10th March, 1875, Mr. LATIMER CLARK, President, in the Chair.

The PRESIDENT said he had to report progress with reference to the Ronalds' Library. He was happy to say that the trust deed had been prepared by the Honorary Solicitor, and had been handsomely engrossed, and was ready to receive the signatures. The books would be brought from Battle to the Society's Library on the following Friday.

The following communication, from Mr. Edison of New York, was read.

ON THE IMPERFECT CONTACTS WHICH OCCUR IN SIGNALLING WITH RIGID CONTACT-POINTS.

By T. A. EDISON.

In a paper by Mr. Latimer Clark describing some experiments on the retardation of signals in submarine cables, printed in the Report of the Submarine Telegraph Committee, a strip of Bain chemical paper is shown upon which is recorded the return static charge from a long circuit formed of buried wires.

On this strip the record, instead of commencing strong and gradually weakening until invisible, shows a distinct break in its

continuity in the first portion of it, as in fig. 1, and this break was observed in all the experiments of this character which he made.

This same phenomenon was observed by myself in some experiments with an artificial cable formed of resistances and condensers, and puzzled me for some time, until the cause was traced to the apparatus employed.

The key which I used for charging and discharging the cable was an ordinary one, with front and back contact-point; the former was connected to the battery; the latter with the recording-point of the Bain instrument, while the lever was connected to the cable.

When the key lever was depressed the cable was charged by contact with the battery; if the key was then allowed to rise the cable was put in connection with the Bain instrument through the back point, and the return charge recorded.

With an iron stylus resting upon ordinary prussiate of potash paper the record was the same as that of Mr. Clark, shown in fig. 1; but when mechanical tailing or elongation of the mark was prevented by using a lead point, and paper moistened in a solution of pyrogallic acid and nitrate of ammonia, the record was that shown in fig. 2.

Fig. 1. 

Fig. 2. 

Fig. 3. 

Fig. 4. 

If more play was given the lever of the key the break was longer, with indications of a second break (fig. 3); by increasing the play the second break is made distinct, as in fig. 4. If the Bain instrument is shunted to decrease the volume of the current, and nearly dry paper used, several distinct breaks are observable; but when the lever of the key is firmly grasped and brought slowly in contact with the back point these breaks did not occur, showing that they are due to the rebound of the lever from the rigid con-

tact-points when brought forcibly against them. A flat spring, adjusted in such a manner as that it would follow the lever up for some distance when rebounding, entirely prevented these breaks.

It follows from this that a Morse dash sent with an ordinary key is not one unbroken wave, but a series of waves, and that the minute breaks which occur, although not perceptible on ordinary relays, cause to be set up in the bobbins of the electro-magnet several inductive currents, which tend to prevent the instantaneous magnetization of its cores. The tongue of a polarised relay in closing a local circuit rebounds in the same manner and gives these breaks. It might at first thought be supposed that the pressure of the hand upon a key-lever would prevent its rebound, but on the contrary the greater the force and pressure used the greater will be the rebound, upon the same principle that it is almost if not impossible to bring a hammer down upon an anvil without several rebounds.

Newark, N. Y., U.S.A.

The President brought before the notice of the meeting a lamp, which was made in Paris, and had been brought by Mr. W. H. Preece. It was provided with a small battery, which, upon pressing a spring at the top heated a small coil of platinum wire, by which a wick, supplied with benzine, was ignited and would continue to burn so long as the benzine lasted. It was, he said, a very convenient lamp for smokers, and as such he recommended it to their notice. It was called the Patent Electric Tinder-box, or Catalyptic Lamp Lighter.

The following Paper was then read :—

BATTERIES, AND THEIR EMPLOYMENT IN TELEGRAPHY.

By JAMES SIVEWRIGHT, M.A., Superintendent, Post Office Telegraphs.

Amongst the many valuable papers which have been read and discussed before the Society, I cannot find that one has been devoted to the general subject of the Battery. This fact has induced me to come forward this evening and bring before you a question which, no matter how it is viewed, whether in a theoretical or practical light, cannot fail to be regarded as one of the highest importance to us.

The contents of my paper must, however, be so well known to almost every one of you, that I should at the outset apologise for bringing them forward at all did I not hope that they will give rise to a discussion which a subject in itself so intrinsically interesting as this can hardly fail to evoke; and that the light of the large and varied experiences of many of you may be shed upon the younger members of the Society who cannot boast of such.

Into the history of the galvanic or voltaic cell I do not propose to enter: from its birth up to the present day two different metals with one or two liquids have, almost without exception, been employed. No sooner, however, was it brought to light than two conflicting theories were advanced as to its action, and these retained possession of the field up to a comparatively recent date.

Volta, in disputing Galvani's opinion that the convulsions of his frog's legs were due to animal electricity, first advanced in 1792 the *contact* theory.

Briefly stated it was this: not only the origin of the action observed in the cell, but the continuation of it as well, is due to the contact of two dissimilar metals; the mere bringing of these into contact begets and sustains a force which is the sole cause of all the energy that is displayed. Volta, and those who afterwards supported him in this view, were ignorant of dynamics. The grand principle

of the Conservation of Energy—the truth of which has now been universally established, but which is in direct opposition to this theory—was to them unknown. They were unable to recognise the fact that the quantity of energy in the universe is constant, that to create *de novo* the smallest fraction of it is as far beyond the power of man as to add one grain of matter to that which is already in existence. All he can do is to alter the form of energy, but in whatever shape it appears it must be recognised as the equivalent of some pre-existing form, which, Proteus-like, is merely driven to assume an altered appearance. And thus to produce electricity there must be a transmutation of energy from some other form, no matter what that form may be—motion, magnetism, heat, or chemism.

In the same year as Volta's ideas were published, Fabroni suggested that chemical action might be one of the causes at work. This gave rise to the *chemical* theory, which, in direct contradiction to the contact theory, asserted that the metals, *per se*, had no power whatever either of originating or sustaining a current, but that the phenomena were entirely the result of chemism, and proportional to the chemical action which took place in the cell. Faraday, with whose name this theory must ever be associated, performed a series of experiments which were not unworthy of *him*, and which, with the apparatus he had at his command, incontestably proved that the view taken by the supporters of the chemical theory was correct. I need but refer to that beautiful experiment of his with the platinum and iron plates immersed in the sulphide of potassium, which is familiar to most of us, and which at the time carried conviction with it as to the truth of the chemical theory.

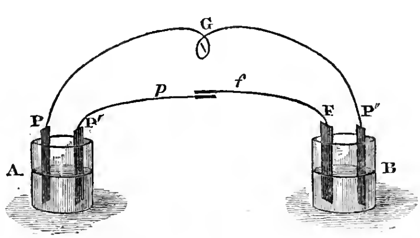


Fig. 1.

A and B (fig. 1) are two vessels, each containing a solution of the sulphide of potassium. This liquid, though a good conductor of electricity, is chemically inactive when associated with platinum and iron in a circuit. Two platinum plates, P and P′, are placed in A; an iron plate F, and platinum plate P″, are placed in B. A platinum wire *p* is attached to the platinum plate P′, and an iron wire *f* to the iron plate F. In the wire connecting P and P″ a galvanometer G is inserted. If the mere contact of *p* and *f* establishes a difference of potential, then all the conditions for a current being present some indication of it would be obtained on G. Faraday found that this was not the case, for the galvanometer gave no evidence of the existence of a current. But on placing a piece of paper moistened with sulphuric acid between *p* and *f* he observed that a decided deflection on the galvanometer ensued.

If, in the wire connecting the outside pair of platinum plates in this experiment, a Thomson's reflecting electrometer of our day had been inserted in place of the comparatively rough apparatus which Faraday then had to work with, there can be no question that that quick eye of his would have detected the fallacy which existed, and his keen intellect would not have rested until a more satisfactory solution had been reached of a problem to which he devoted so much of his time and power—a problem which even to the present day, notwithstanding the many efforts which have been made, still waits for a satisfactory explanation.

The latest theory which has been advanced goes a long way towards reconciling the conflicting opinions which were entertained by the supporters of the contact and chemical theories. For it allows to the former that the initial action is due to the simple contact of dissimilar bodies, and to the latter that this action can be maintained only by chemism.

The fact is now unquestioned that the mere contact of two dissimilar metals throws them into different electrical conditions—or in other words determines a difference of electric potential betwixt them. And not only is this the case with dissimilar metals; the contact of a metal and a liquid produces the same

effect to a different degree, although, strange to say, if two dissimilar metals are plunged into the same liquid the phenomenon is not observed; the metals and the liquid then appear to remain in precisely the same electrical condition. Whether this is actually the case, or whether from defective means of observation it only appears to be so, it is somewhat difficult to say, although it would seem that Sir William Thomson's experiment, first made public in Professor Fleeming Jenkin's "Electricity and Magnetism," is really conclusive on the point.*

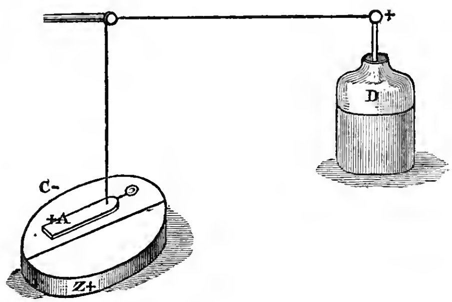


Fig. 2.

Over two half-discs (fig. 2)—one of copper, C, the other of zinc, Z, and placed symmetrically with respect to them—a flat strip of metal, or needle A, kept charged at a high potential from a source D, is suspended. So long as the discs are in no way connected with each other the needle remains at rest, but immediately they are brought together or united by a metallic wire the needle is deflected in a direction which varies according to the nature of the charge communicated to it. If its charge is reversed so also is the deflection. In place of by the metallic wire let the connection between the half-discs be made by a drop of water; the needle then returns to, and remains at, rest, no matter what its charge may be, positive or negative.

Although the two metals—copper and zinc let us say—and the liquid in an ordinary cell may all remain in the same condition, yet, if a copper wire be employed to connect the metals with each

* Jenkin's "Electricity and Magnetism," p. 45.

other, then at its junction with the zinc plate a difference of potential is at once determined, just as if the two plates themselves had been brought together. The zinc has the higher potential, that is, is positively electrified; the copper wire the lower potential, that is, is negatively electrified. There being little or no opposition to an equalisation of these conditions, there is an apparent transfer of electricity, or in other words a current sets in, and is maintained from the point of higher potential to the point of lower potential.

But in order to produce and keep up this current, which is capable of doing work, there must be a consumption of an equivalent amount of energy in some other form.

In finding this equivalent lies the weak point of the old contact theory as well as of the modern theory of the action of the cell. The mere contact of the two dissimilar metals throws them into different electrical conditions; this is accompanied by work, or the power to do work. Yet, so far as we can judge at present, there has been no transmutation of energy in order to produce this.

Whether or not it may be due to some re-arrangement in the ultimate molecules of matter,—whether the power which is thus suddenly called into existence may or may not be the result of a clashing together of the atoms of the two metals when they are brought into contact,—it is impossible to say. Nor does it seem as though experiment could ever avail us much in these provinces: the aid of that grand faculty, the scientific imagination, will in all likelihood have to be invoked in order to cap the theory, and give to it that consistency with modern research which at present it can hardly be said to possess.

The maintenance of the current is satisfactorily accounted for by the chemical action which takes place in the cell, whereby in the decomposition of the liquid hydrogen is evolved, and the zinc burnt into an oxide. There is here a transmutation of one form of energy which reappears in another in the galvanic current. In fact, from the energy of the chemical action which takes place in the cell the strength of current produced can be estimated.

In every battery, therefore, chemical action must take place, that is to say, one or other of the elements composing the liquid must possess the power of combining chemically with one or other of the metals employed in it. And it is to this chemical action, which is indispensable, that the main defects to be met with more or less in all forms of galvanic batteries are due. Zinc, which is almost invariably employed as the positive element, is oxidised; hydrogen accumulates on the negative element. But the mere contact of hydrogen and copper determines to the former a positive potential, and causes it to behave in the same manner as the zinc, that is to say, a current is set up from the hydrogen on the copper plate through the liquid in the reverse direction to the original current. The resultant electromotive force is thus weakened and rendered variable according as the hydrogen forms and recombines with the oxygen liberated by the secondary action.

This *galvanic polarisation*, as it has been unfortunately named, is therefore the first evil to be surmounted in any battery which is put forward as fit for general practical use. To get rid of this antagonistic electromotive force is, it is needless to remark, the main object of all the so-called *constant* batteries.

Another evil to which it would be well to draw attention before proceeding to look at some of the forms of constant batteries is that which is known to us under the name of *local action*. This is mainly due to the impurities in the shape of foreign ingredients in the metals which are employed. Take for instance a zinc plate containing small quantities of some other metal, let us say iron, and immerse it in a liquid; all the conditions for a current and the promotion of galvanic action between these are present; short circuits are therefore formed and needless waste ensues.

There are three leading conditions which should be fulfilled by a perfect battery for actual every-day use:—

1st. The current obtained from it should be constant.

2nd. When the battery is not actually required there should be no action going on in it, attended by a needless consumption of material.

3rd. The materials employed in its construction and maintenance should not be expensive, or, at all events, should be as inexpensive as possible, and there should be no difficulty or danger in handling them.

There are various other collateral points which will be touched upon, but these, I think, include the main conditions, which, if they were only realised, would place us in possession of a perfect battery. The importance attached to each is so self-evident, that it is not necessary to dwell upon them.

Taking now some of the forms of batteries which are employed throughout the world, let us inquire how far they come up to these conditions, and in what respects they fall short of them.

I shall commence with the Daniell, not more on account of the time-honoured position which it still occupies in England, than from the fact that either in its original principle, or in one or other of its numerous modifications, it is that which, with very few exceptions, is most generally employed in those countries where a system of telegraphy has been established.

The essential feature of Daniell's battery is the employment of two liquids instead of one, separated from each other by a porous diaphragm. Surrounding the copper plate with a solution of sulphate of copper, he provided for the deposition of copper in place of the liberation of hydrogen upon it, and thereby effectually got rid of galvanic polarisation. The application of this principle to the battery employed in England for telegraphic purposes was, I believe, first made by Mr. John Fuller; the sulphate form of battery introduced by him replaced the defective sand-batteries made use of in the early days of telegraphy, and differed in no material respect from that which is here exhibited.

The sand-battery was the simple galvanic couple, zinc and copper, plunged into a solution of water acidulated with sulphuric acid in the proportion of twelve parts of the former to one of the latter. The addition of sand to this—the discovery in fact of the sand-battery—was, like so many other discoveries, the result of what is generally called accident, but what Mr. Smiles would probably call "an opportunity improved by genius." Sir—then

Mr.—William Fothergill Cooke, while testing some of the lines, had occasion, it is stated, to place the testing battery during its conveyance from one station to another in a truck containing a quantity of sand. Some of the sand accidentally found its way into the battery. The working was, however, in no way interfered with, and from that day up to the introduction of the sulphate the sand-battery was the practical form of battery employed in England.

Into a historical survey of the batteries, however, extremely interesting although the subject is, I cannot enter; my object is rather to consider the points of advantage which may be claimed by those now in actual use, as well as the weaknesses which are incidental to or inseparable from them.

The ordinary form of Daniell which is now commonly employed is here shown. The trough shape is adopted for the simple reason that it can be easily handled, and a greater number of elements can be put into the same space in this way than in any other. Teak is employed in the construction, on account of its durability, and the little tendency which it exhibits to warp. The trough is divided by slate partitions into ten cells, and each of these is again subdivided by a plate of porous unglazed porcelain. Previous, however, to the insertion of the porcelain the trough is coated with marine glue, for the purpose of rendering it perfectly watertight, and at the same time providing against any leakage from one cell to another. Notwithstanding the precaution which is thus taken, the complaint of a "leaky cell" is one of the most common of the few faults that are to be found with the battery. This may be due either to the marine glue being of inferior quality—an occurrence which is by no means rare now that the commercial value of shellac, one of its constituents, has risen so much of late years—or to the process of heating the glue and coating the trough being imperfectly carried out. In many cases, for instance, the slates are inserted before the trough is glued; the danger is thus incurred of leakage taking place between two adjacent cells by the liquid making its way round the edges of the slates and through the interstices between them and the trough. Although longer time may be occupied in serving the trough with marine glue, previous

to the insertion of the slates, and thereby coating the grooves as well as the sides with it, still the risk of leakage is considerably reduced, and the comparative immunity from danger to the working of the battery owing to this cause fully compensates for the increased cost of labour. The porous partitions are next inserted, and by canting the trough from side to side once or twice they are similarly served along the edges and for a short distance up their surface with the glue. The battery plates are then put in, crystals of sulphate of copper are placed in the copper cell, and water is filled in to both it and the zinc cell up to within a short distance of the top of each plate.

Passing by any inquiry into what the size of the battery-plates should be, it is worthy of remark that the results obtained from the use of zincs cast in a closed mould—a practice which is now beginning to prevail more widely than formerly—are more satisfactory than those derived from the employment of zincs cast in an open mould. The compactness and comparative homogeneity of surface which the former present argue directly in favour of the closed mould; the plates cast in it are more evenly consumed, and must thus contribute to the constancy of the battery.

Constancy is the main feature of Daniell's battery; in this respect it has never been surpassed by any of its rivals, and answers more fully than any other the first condition of a good practical battery for telegraph work.

But in the second condition it falls far short of the ideal battery; the enormous waste of material which takes place, owing to the two liquids, in virtue of the property of *osmose*, steadily diffusing into each other, is the main drawback to the Daniell. The chemical action which is constantly going on is out of all proportion to the amount of real work done, and only in cases where an incessant demand is made upon the battery can even an approach to the full amount of the energy which is displayed be utilised.

Various efforts have been made to reduce this action as far as possible, but it is inherent to every battery in which two liquids varying in specific gravity are employed; and it must therefore be looked for to a greater or less extent in all of them. It has been suggested that the porous partitions might be made thicker; this

would no doubt impede to some extent the mixture of the two liquids, but it brings at the same time increased resistance, and beyond a certain limit introduces a greater defect, into the battery than that which it is intended to remedy.

Conflicting opinions are entertained as to the effect which the current exercises upon this osmotic action. Mr. Culley says, "The liquid passes through the porous cell from the zinc to the copper in virtue of a singular property common to all porous substances when dividing dissimilar liquids, called 'osmose,' and it will frequently rise in the copper cell an inch or more above the level. The current aids this movement."*

Mr. Sabine, on the other hand, states that the destruction of the element from the diffusion of the two liquids "takes place faster during the time the circuit is open than when it is closed."† And in *The Electrician* the same opinion is expressed in one of a series of articles upon "The Electric and International Telegraph Company," by a writer who goes a step farther, and in speaking of closed circuits says: "In this system, when the line is not employed in transmission, a continuous current from the battery is allowed to pass, permanently deflecting the galvanometers at all stations. When a message is to be transmitted from any station, the operator at this point interrupts the current along the wire, or breaks the circuit by moving a small brass lever attached to the key, and immediately all the galvanometers along the line return to zero. * * * * It might be supposed that the continuous action of the battery would be attended with a great expenditure of material. This, however, does not appear to be the case; the increased consumption of zinc in the production of voltaic effect being compensated in a great degree by the diminution of exosmosis of the copper solution in the porous vessel of the battery cell while the battery is in action, the escape of this solution and its action upon the zinc element being a source of waste, which in practice is greater than the legitimate wear and tear of the battery."‡

* Culley's *Practical Telegraphy*, 6th edition, p. 16, sec. 41.

† Sabine's *Electric Telegraph* (1867), pp. 223, 224.

‡ *The Electrician*, vol. i. No. 14, Feb. 7th, 1862.

In the event of the closed circuit ever being to any extent again introduced into England, this becomes a point of vital importance, for one of the main arguments urged against its adoption is the waste of battery power which would necessarily ensue on many circuits. If, however, the consumption of material in Daniell's battery is actually less when the circuit is closed than when it is open, here is a direct argument in favour of its introduction. In America this system is generally employed, and the difficulties which are there encountered in the matter of adjustment—so fruitful a source of trouble in the single current working with several instruments in an open circuit—are, it is stated, so slight, as practically to afford little or no inconvenience.

As regards the third condition, cheapness in construction and maintenance, combined with ease in handling, this form of Daniell's battery may be said to hold its own; a ten-cell costs 1*l.* 1*s.*, and the average cost of the materials consumed in it per annum may be set down at 8*s.*

The most dangerous rival which Daniell's battery has ever had to contend with is that invented by M. Leclanché, of the Great Eastern Railway of France.

It has been tried in England for five years or more, and since its introduction has to a great extent ousted the Daniell and the other forms of two-fluid batteries in use at the time from the position which they then held; in fact, except the Daniell, I am not aware that there is one now left to compete with it. Leclanché's battery is a one-fluid battery. From a consideration of the unnecessary waste of materials which takes place in all the two-fluid batteries, and in none perhaps more than in the Daniell, Leclanché was led to seek for a battery in which the chemical action would be perfectly equivalent to the work done. This he hoped to have found by using the peroxide of manganese in presence of ammoniacal salts. Zinc immersed in a solution of chloride of ammonium—the ordinary *sal-ammoniac* of commerce—is the positive element. Peroxide of manganese was adopted as the negative element; the readiness with which it yields up a portion of its oxygen when brought into contact with combustible bodies renders it very suitable for this; in

the form known as *needled* manganese it is mixed with an equal volume of crushed horn carbon: into a porous pot containing this mixture a plate of carbon is placed and made to play the part of a collecting plate for the electricity which is generated; the connecting wire of iron is let into the zinc, and attached by means of a brass-binding screw to the lead cap which is fitted on to the carbon plate.

As soon as the circuit is completed chlorine (Cl) is liberated at the positive plate, and forms with it chloride of zinc ($Zn\,Cl_2$), an extremely soluble salt; the radical ammonium ($N\,H_4$) is readily burnt, the products of combustion being water ($H_2\,O$) and the free ammoniacal gas ($N\,H_3$). The action which takes place in the cell may be symbolically represented thus:—

Before contact—Zn, $2(NH_4\,Cl)$, $2(Mn\,O_2)$, C.

After contact—$Zn\,Cl_2$, $2NH_3$, H_2O, Mn_2O_3, C.

Looking at this, nothing could be considered more simple; the action is to all appearance theoretically perfect, and the many defects of previous batteries would appear to be banished from that now before us. Experience and practice have demonstrated that this is not so; but have shown on the contrary that Leclanché's battery, although far superior to all the other single-fluid batteries that have ever been tried, yet labours under some at least of the disadvantages to which they are subject. So long as nothing but the action which is indicated above takes place, so long is the working of the battery all that can be desired. But secondary actions soon begin to show themselves if a continued strain is kept upon it. Galvanic polarisation ensues, and the internal resistance of the battery rises until from both these causes its constancy is seriously impaired. This at least seems to be the only explanation which can be put forward for the facts, which are now generally acknowledged, that Leclanché's battery cannot be relied upon for the maintenance of a constant current; that it is not adapted for a local; and that on a very busy circuit it cannot be depended upon. Galvanic polarisation would arise from the "turbulent" hydrogen, as Leclanché has well named it, accumulating on the negative plate, its presence there being in all probability due to the same

cause as on the carbon in the Marié Davy. The ammonium (NH_4), breaking up into ammonia (NH_3) and hydrogen (H), may be liberated in too large quantities for even such an oxidising agent as peroxide of manganese (MnO_2) to be able to effectually get rid of the latter: whether this difficulty might not to some extent be overcome by increasing the quantity of peroxide of manganese, or what effect this would have upon the action of the battery, I cannot say.

The internal resistance of the cell varies from the formation of the double salts, oxychloride of zinc and zinc-ammonic chloride; longer time and a more concentrated solution of sal-ammoniac are required to dissolve these than in the case of the simple chloride of zinc. It is for this reason that Leclanché recommends the use of a "strong saline solution" in the zinc cell. When the battery is allowed breathing space, so to speak, and is permitted to rest for a short time, the hydrogen, and with it galvanic polarisation, disappear; the double salts are dissolved; and the battery regains its original strength with wonderful rapidity.

This want of constancy is the great defect of Leclanché's battery for general use, and in this respect it cannot for a moment stand in comparison with Daniell's; but as regards the second condition, which it was suggested that a practical battery should fulfil, the Leclanché battery seems to leave nothing to be desired. With the single fluid no osmotic action can take place, and when the battery is at rest we had almost said that it actually is at rest. Still this statement cannot be made without some reservation; for, although no consumption of materials may be needlessly taking place as in the Daniell from a diffusion of the liquids, still in Leclanché's battery an action is going on which in addition to destroying some portion of the cell lays the foundation for indifferent working, and may ultimately end in a total breakdown. I allude to the local action observable in the first forms of the cell at the point where the brass screw, iron wire, and lead cap are united together, as well as to the formation of the cloride of lead ($Pb\,Cl_2$) where the carbon and lead come into contact with each other. Two different metals, without the presence of an alloy, would have been

sufficient to account for the action which takes place at the connecting screw. To get rid of the disconnections occasionally caused by the metallic salts which are formed at this point, the iron wire is now often welded into or soldered on to the lead cap. This method is so far successful in effecting the object which it had in view, but no plan has yet been devised which will prevent the formation of the chloride of lead.

Instead of the brass screw being screwed into the lead cap, the screw is sometimes welded into the cap, and the connecting wire affixed to it by means of a brass nut. This latter plan seems to answer very well and is certainly superior to the former, where, after the screw has been several times removed from the lead, the hole into which it is fitted becomes too large to admit of any catch being made upon it.

The lead cap may be fitted as closely as it possibly can be on to the carbon, and the precaution may be taken of dipping it afterwards into melted paraffin until the paraffin makes its way for some distance beyond the lead; still the peculiar action evidenced by the white chloride of lead sooner or later makes its appearance and necessitates the removal of the pot.

The corrosion of the iron connecting wires by the fumes of free ammonia given off in the action of the battery was a source of complaint for some time, but it has been successfully met by carefully covering the iron with some unoxidisable material, Chatterton's compound, india-rubber, gutta-percha, tar, paint, or grease.

The glass jar, from its liability to fracture, is an objectionable feature in the mechanism of the battery; this, however, is purely a mechanical defect, if, in fact, so strong a term can be applied to it, and can, if required, be easily surmounted.

But the main source of trouble and expense in the maintenance of Leclanché's battery is the porous pots—the peeling and bursting of these seem to be unavoidable; the formidable array which I have collected during the last ten days or a fortnight, and which is now before you, is an indication that "something is wrong somewhere." Before endeavouring to fix the responsibility for the breakage of these let us inquire to what it can be attributed.

We have seen that in the action of the battery the chloride of zinc, an extremely soluble salt, is formed: that oxychlorides and zinc-ammonic chlorides, also soluble, but still to a less extent, are likewise formed. In the liquid holding these and sal-ammoniac in solution the porous pot stands and is gradually permeated by it. Through the pores of the earthenware the liquid makes its way. The double salts, being now to some extent freed from the grasp of the concentrated solution of sal-ammoniac, exhibit an irresistible tendency to return back to the solid form, and in doing so they crack the pot in a manner exactly analogous to the bursting of water-pipes during a frost. Many of these porous pots when broken up show a casing of salt around the manganese compound resembling a thin sheet of ice; subjected to a chemical test, this proved to be a double chloride of ammonium and zinc, and was in fact exactly the same salt as that scraped off the zincs of a battery that had been working for some time on a moderately busy circuit.

It is evident, also, that the cracking of the porous pots may be, to a certain extent, due to the evaporation of the liquid; the salt previously held in solution, no matter whether sal-ammoniac or any of the chlorides, will pass into the solid form, and possibly do some damage to the porous earthenware. But the danger to be anticipated or actually arising from this cause is but slight compared with that which is occasioned by the formation of the double salts; could their presence be effectually got rid of, Leclanché's battery might be employed to a far greater extent than now, and not only would there be less fear of its giving out at some critical moment but the cost of maintenance, even in its extended application, would be largely reduced.

The introduction of the trough-form of Leclanché's battery, similar in appearance to the ordinary sulphate trough, was intended to get rid of the defects which have been alluded to in the earlier issue, and to some extent it has succeeded in accomplishing the object which it had in view. The abolition of the glass jars entirely is one point in its favour; another is the small length of connecting wire which is now rendered necessary; with it there is less liability than with a longer length of any interruptions being caused by the

wire being corroded. The porous partitions are likewise preferable to the pots; they are not burst by the solidification of any liquid coming from the zinc into the carbon cell. But the peeling or flaking prevails in this as much as in the pots, and seems to be inherent to the principle of the battery. I have brought here to-night a Leclanché trough, and it is interesting to note the condition in which the partitions are: from being 3-16ths of an inch in thickness they have crumbled away to such an extent that in some places there is not more than a mere skin left. This battery was joined up with two others upon the 12th of May, 1874, to work a moderately busy printing circuit, offering a resistance of 2,700 ohms; they gave every satisfaction. On the 14th of August—that is, three months after—a little water was added to the zinc cells, and two of them were joined up as line batteries on a sounder circuit, giving a resistance of 1,900 ohms. The third—that which is now before me—was kept spare, ready charged, however, all the time. On the 26th of November, after being well refreshed with sal-ammoniac and water, it was tried as a "local" to work a fairly busy sounder; the resistance of the circuit was 21 ohms. It commenced splendidly, and completely threw into the shade sulphate batteries of the chamber form, which were working three other sounders on the same bench; but on the 9th of January, during the receipt of a batch of five or six messages, it failed; the receiving clerk began to complain of losing dots after he had taken off the first three, and the battery, on being tested for quantity, almost refused to shake the needle of the detector. Gradually it improved, when left to itself, without anything whatever being done, and was tried on another local circuit, offering the same resistance, but with comparatively little traffic passing along it. Here the battery did all that was required of it up to the 2nd of February, when, upon examination, the porous partitions were found to be peeling away. It was then removed, refreshed with sal-ammoniac, part of the zinc solution drawn off and replaced by water, and on the 9th of February it was again tried on the original local circuit. On the 23rd of February it failed a second time in exactly the same way as before—during the receipt of a

batch of messages. On being removed, and the liquid withdrawn, it was found to be in its present condition.

But others which were supplied at the same time as this (including the two which worked along with it) and were joined up, some on single needle circuits, others on moderately busy printing circuits, have given every satisfaction; beyond the scraping of the zinc, the removal of the liquid from the carbon cell, and the replacement of a portion of that in the zinc cell by pure water from time to time—certainly not oftener than once in three months—nothing whatever has been done to them, and when examined yesterday they showed no indication of giving out, but, on the contrary, appeared to be in as good condition as on the day when they were first joined up. Upon such circuits as these, where the traffic is not very heavy, as well as for the ringing of bells, whether for signalling or domestic purposes, Leclanché's battery is without a rival, and for purposes of this nature it may be said to have fairly driven the ordinary sulphate from off the field. In fact, so well is work of this kind performed by the Leclanché, that it is impossible to speak too highly of the manner in which it is done, and we cannot but regret that the battery in its present form is not adapted for universal employment in telegraphy.

In point of expense Leclanché's battery contrasts favourably with any other form of battery that has ever been in general use. For although at first sight the prime cost may appear in excess of the Daniell—an 8-cell Leclanché costing 36*s.*—still it must be borne in mind that the strength of the current obtained from the former is far in excess of that from the latter. Leclanché himself states that "in practice 40 Daniells are replaced by 28 of mine;" but he is if anything below the mark. In the case of the trough form of Leclanché's battery already referred to, 16 cells are now employed to do the work of 30 of the ordinary sulphate cells, and they have done it equally well. The prime cost of both may therefore be set down as about the same. As regards maintenance, the Leclanché, where it can safely be employed, is cheaper in every respect, for not only is the quantity as well as the cost of

materials consumed in it far less, but the labour attendant upon its refreshing and renewal is not so great as in the Daniell. The materials which the lineman has to carry—now that he is freed from the danger of meeting with broken glass jars or cracked porous pots—and the time occupied by him are for the trough Leclanché far below the demands which, under similar circumstances, would be made upon him by the trough Daniell.

Before quitting the subject of Leclanché's battery, it may be well to mention that efforts have been made in various quarters to employ common salt in place of sal-ammoniac, but none, so far as I can learn, has ever been attended with success. Leclanché himself tried some of the sodium salts, but the results which he obtained were such as to discourage him from persevering with them. The sodium which is freed, taking up oxygen at once from the water, forms soda; the hydrogen is unconsumed by the peroxide of manganese, from the fact, according to Leclanché, that free hydrogen is not nearly so combustible as ammonium in the presence of peroxide of manganese; the consequence is, of course, galvanic polarisation with its attendant evils.

The large cells on the table formed a portion of the elements of a 10-cell battery charged with common salt, in place of sal-ammoniac, which was joined up on the 18th of last June to work a single needle circuit offering a resistance of 1,940 ohms. At the close of two months half the liquid was drawn off and replaced by water. It was put back, but at the end of three weeks the complaint of weak signals began to be heard, and the salt-battery had to be removed. It was then tried by itself on a minor single needle circuit having a resistance of 954 ohms, on which very little work was done, and in anything but fine weather it proved itself unequal even to this. It was, therefore, discarded from actual working; and although several efforts were made from time to time, by cleaning the zincs—which, it was observed, got rapidly blackened—and adding fresh salt, to get the porous pots to work, they persistently refused to do anything more than give just a sensible shake on a lineman's moderately-sensitive detector.

To get rid of the inconvenience of the porous partition in Daniell's

battery advantage was taken of the difference in the specific gravities of the two solutions in the sulphate battery, and a battery was thus introduced which relied upon the force of gravity alone to keep them apart.

The germ of this idea may be seen in Fuller's battery; Mr. Cromwell Varley was the first who patented it.

The "gravity" form of Daniell's battery is but little employed in England now, although, in one of its many modifications, it is in general use in India, in America, and on the Continent. The perfect rest essential for its proper working, so as to prevent the mixture of the two solutions when separated by gravity alone, led to its being abandoned for all except testing purposes.

In India the Minotti is all but universally used; the mode of setting it up, as well as its treatment there, have been already explained to the members of the Society by Mr. Ayrton, in his paper upon "The Indian Telegraphs." Combined with most of the points that may be urged in favour of the ordinary form of sulphate battery, the Minotti possesses the additional advantage of being portable from place to place, without the necessity of removing any of the liquid employed in it. For this reason it is adopted by the Postal Telegraph Department in the travelling vans, whose appearance at many of the special gatherings is familiar to most of us. To no more crucial test, setting aside for the moment the question of cost, could a battery intended for universal use in telegraphy be subjected than when joined up on one of the circuits worked in connection with these. The Minotti has passed through this ordeal to the satisfaction of every one, and never failed to answer the incessant demands which were made upon it, whether for single current working, double current working, line, or local.

The objection which has been urged against its being more widely employed in place of the ordinary form of sulphate battery, is the increased cost of its maintenance; statistics in support of this objection I am not in a position to give.

Grove's form of constant battery consists of a plate of platinum in concentrated nitric acid as the negative element, and a plate of zinc

in dilute sulphuric acid as the positive element. Galvanic polarisation is here prevented by the hydrogen being burnt into water by the nitric acid; the latter is in time entirely reduced, and nitric oxide is given off, which, immediately on coming into contact with the air, combines with oxygen and forms the poisonous red fumes of the tetroxide of nitrogen. Great electromotive force is obtained from the combination employed in Grove's battery; its internal resistance too is very low, and consequently the current from it is far in excess of that from Daniell's battery. For experimental purposes, the battery is all but invaluable; for general practical working it is altogether unsuited, and was never, so far as I can find out, intended for such. It possesses none of the qualifications which such a battery should possess. The current obtained is not constant for any length of time; there is a needless waste of material on account of the vigorous chemical action which is constantly going on; the materials employed are not only intrinsically dear, but they are difficult to handle; and the expense of attending to the battery, apart from the actual cost of these, is very great: for it requires constant looking to, and every night, when not required for active service, it has to be taken to pieces.

Yet notwithstanding all this, Grove's battery was until quite recently employed to a great extent in America. There the circuits are largely worked on the so-called "Universal Battery" principle, that is to say, several of them are served by the same set of batteries, and the heavy wires of the Western Union Company leading from the principal cities were in this way worked by Grove's elements.

The carbon, or as it was also termed the electropoion, battery was similarly employed in America for some time. It resembled Bunsen's battery, in having carbon or coke as the negative plate, but differed from it in having in place of concentrated nitric acid a solution of the bichromate of potash. This solution was made as follows: Three gallons of water were mixed with one of sulphuric acid. In a separate vessel 5lbs. of the bichromate of potash were dissolved in two gallons of boiling water. These two were then thoroughly mixed, and the resulting solution, in which heat was

developed by reason of the chemical action that took place, was allowed to cool previous to being used. When this battery was at work a third of the bichromate solution had to be removed and replaced by a fresh supply every morning; the amalgamation of the zincs, a point of vital importance in both this and Grove's battery, had constantly to be seen to; and at the end of a fortnight every cell had to be taken to pieces, the zincs brushed, the carbons soaked in clean water, and the sulphuric acid solution renewed.

This, although an expensive battery, had the advantage of cheapness both in materials and attendance over Grove's form, the relative cost of the former being less than one-third that of the latter.

Both have now had their day, so far as general practical working for telegraphic purposes goes, and will in all probability be speedily numbered amongst the experiences of the past. The wonder really is, how in the face of the other forms of batteries they could ever have stood their ground so long and so well as they have done.

They are fast making way for one or other of the many forms of Daniell's battery, which mainly owe their separate existences to the indomitable perseverance in patenting which characterises the Transatlantic Telegraph Engineers. Time does not admit of my doing more than making a brief allusion to one or two of the numerous modifications of the sulphate battery which have been tried there—the Hill, the Lockwood, the Baltimore battery, the Callaud, &c.

The last named, the Callaud, introduced in France by Mons. Callaud, appears to be that which is now coming into most extensive use in America. It is a gravity battery pure and simple. The copper plate is placed at the bottom of the cell, in some in the form of a flat plate, in others with the corners turned down, to serve as legs and keep it slightly raised above the bottom of the jar, whilst occasionally a coil of copper wire is employed. The zinc has likewise more than one form. Sometimes it is star-shaped; at other times it is a perforated plate; and not unfrequently it is conically shaped on the lower surface, so as to prevent the bubbles of gas from adhering to it. Callaud proposed to vary

the internal resistance of his cell by providing for the lowering or raising of the zinc plate at will. Experience has pronounced this method to be entirely useless, and from the loosening of the binding screws, accompanied by the consequent dropping of the zincs, to be actually a detriment instead of an advantage to the battery.

The Callaud has all the disadvantages of the gravity battery; complete rest is essential for its efficient working. The treatment is simple enough, and the mixture of the two solutions, whilst that around the copper plate is preserved at the required strength, is the main point to be guarded against.

Mr. Haskins, of the North Western Telegraph Company of America, writing to the American Journal of the Telegraph, on the 16th of September, 1873, gives as the result of his experience the following rules for the treatment of Callaud's battery:—

"Drop a piece of vitriol of the size of a walnut into each jar when it is set up. Let this dissolve, then add small crystals, until the blue solution is (say) two inches high from the bottom of the jar. Instruct your battery-man to add the crystals daily, keeping the solution as near as possible at the above height. Wipe the outside of the jar, and the inside, above the fluid, daily, to remove the salt of zinc that may be deposited there by evaporation. When the sulphate of zinc solution becomes syrupy, like strained honey, or begins to deposit crystals on the zinc disc, draw off half of it with a syphon made of twelve to eighteen inches of soft rubber tubing, and fill with fresh water. See that fresh water is added, to replace that lost by evaporation."

The introduction of a few drops of linseed oil, which covers the solution with a fine film, is sometimes resorted to in order to prevent evaporation from taking place. He adds:—"If batteries are managed in this way they do not need cleansing or taking down. One battery working two wires was set up March 17, 1872, another with four lines July 1st, same year; they are both Callaud's; neither has been taken down, and they are still clean and working well."

Mr. Jones, of the Western Union Company, writing to the same journal on the 1st July, 1874, upon the subject of "Callaud

Batteries," says: "The cost per month for 600 Callaud cells, comprising 3 batteries, working 10 circuits of an average resistance of 5,000 ohms, is nearly 30 dollars, or 5 cents each; and for 260 local cells, which remain in closed circuit when not being opened by key use, the cost is about 14 cents per cell monthly." And again: "To place beyond doubt the feasibility of working several wires from a Callaud battery, I worked for seven days and nights fifteen important railroad and commercial circuits, varying in resistance from 4,000 to 11,000 ohms, from 180 Callaud cells, with fine results. At the time the wires were first attached to the battery the sulphate of zinc solution stood at 13 degrees; at the end of the seven days it stood at 15 degrees, without any addition of water. At night some of the circuits were reduced one-half in resistance by relays being cut out. A prettier Callaud battery could not be found; the sulphate of copper solution never rising over an inch from the copper plate, and as deep and handsome in colour as an Italian sky. The material consumed in seven days was 17 lbs. of zinc and 62 lbs. sulphate of copper, which, at market value, equals 9 dollars 20 cents. The cost of 180 Callaud cells working two circuits of about 6,000 ohms during the same space of time was about 2 dollars 30 cents. By working down a Callaud cell you accomplish just what is designed by the spiral wire in the Lockwood."

These are no doubt valuable facts in support of this form of battery, but, when it is borne in mind that all this has been attained only as the result of daily attention on the part of the battery-man, they lose much of their significance, and, although I have not heard of the experiment ever having been tried, there can be no doubt that, were one of the ordinary form of batteries employed in England watched over with the same fostering care, it would be an exceedingly difficult matter indeed to assign a limit to the time that it would remain at work without being "taken down." This, however, for self-evident reasons, is all but entirely out of the question in England, and, such being the case, the Callaud battery may be dismissed without further comment.

Reverting for a moment to the subject of closed circuits, it is

worthy of remark that the figures given by Mr. Jones as to the relative cost of maintaining Callaud's cell on open and closed circuits do not support the idea that any advantage can be claimed for the latter so far as the consumption of materials is concerned. The cost of each cell per month, when in open circuit, was 5 cents, when in closed circuit 14 cents.

The feature of the Lockwood battery, so named from its inventor —the battery-keeper to the American District Telegraph Company in New York—is the arrangement of the copper element, and is so novel in its way that I have had roughly-executed drawings prepared showing the arrangement of one of the cells (fig. 3).

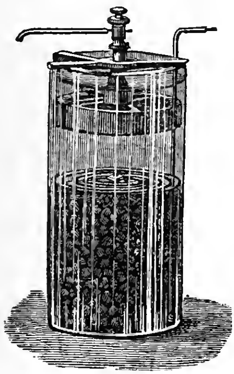


Fig. 3.

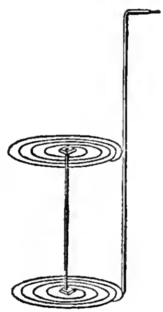


Fig. 4.

The copper element (fig. 4) consists of two concentric coils of copper wire and an upright standard, formed of a straight piece of heavy copper wire, provided with nuts and washers at each end. An insulated wire is connected to the lower coil, and passed upwards through the jar to the succeeding zinc. Special stress is laid upon the necessity of so arranging this that the outside of the copper coil should run to the *right* of the insulated wire; and at the same time care must be taken that the coils are so placed that the outside of the lower coil runs to the *right*, and the outside of the upper coil to the *left*. This, it is stated, is essential to the proper action of the battery. The result of all this is, that when the cells are charged in the usual way, and "the battery is placed in circuit, the influence of the electric current passing through the coils is such as to keep the blue solution entirely below the upper coil. If the battery is

working properly, in a day or two the upper edge of the blue solution below the coil will be sharp, distinct, and well defined, while the solution surrounding the zinc will be beautifully clear and transparent. If the battery is left too long on open circuit, the copper solution may sometimes rise above the upper coil, but on closing the circuit it will be gradually drawn down to its proper place. The battery will work well with a space of one inch between the zinc and the upper coil, which gives a very low internal resistance and large quantity. Where this is not required, it is perhaps better to allow more space between the elements."*

This battery is employed to a considerable extent in America, and is said to work on the whole very well; whether or not there is any special virtue in this arrangement of the copper plate, I shall leave for the present an open question.

The Baltimore battery, the invention of Mr. Davis of that city, has likewise been patented. The essential elements are the same as in all gravity sulphate batteries. Novel features claimed for it are the shape and supports of the positive element, a feeding-tube with a perforated cork down which the crystals of sulphate of copper are dropped, so as to avoid disturbing the liquid in any way; but in all these there does not appear to be anything of sufficient importance to recommend the adoption of this in preference to the better known forms of gravity battery.

On the Continent the most important modifications of the Daniell battery have been made. Kramer's, Meidinger's, and Siemens's have all their special advantages. The last named especially commends itself where a form of sulphate battery is required. The improvement effected by Messrs. Siemens and Halske in the diaphragm obviates to a great extent, and for a considerable time after the cell is charged, the evils arising from the action of osmose. Paper pulp is the substance which is employed as the diaphragm; this undergoes a special preparation previous to being introduced into the battery. It is treated with about one-fourth of its weight of sulphuric acid and afterwards soaked for

* Extract from the pamphlet published by the proprietors of Lockwood's battery.

a few minutes in water. The liquid is then pressed out, and the pulp is packed firmly around a bell-shaped vessel of porous porcelain, in which the copper element and sulphate of copper are placed.

A battery fitted up in this way is as reliable a form of Daniell as can be found for use upon a circuit offering considerable resistance.

The work done by it and the manner in which that work has been done are the most powerful arguments that can be adduced in its favour, and, even if time admitted, require nothing to be said by me in support of them.

In this fragmentary glance at some of the batteries employed in telegraphy I am but too conscious of the many imperfections which exist; many points of the utmost importance in connection with the subject have been either treated very lightly or passed over entirely; some intentionally, as for instance the appropriation of battery power to the various circuits—a question that would well repay any one who made it the subject of an evening's discussion. All I can hope is, that what has been said may elicit an account of the experiences and the practical suggestions of many of you; for, notwithstanding the many improvements that have been made, and are daily being made, in the principle and construction of batteries, the perfect battery for universal use—if anything in connection with telegraphy will ever be accounted perfect—is still a thing of the future, and must, when it eventually does make its appearance, realize on the one hand the principle aimed at by Leclanché, of giving us in the work done a complete equivalent for the materials consumed, and inspire on the other hand the perfect confidence of the Daniell, in being equal to any emergency, and ever ready to respond to whatever demands are made upon it.

Mr. Sivewright then explained the various forms of batteries exhibited on the table.

The President: We have all listened, I am sure, with great interest to one of the most valuable papers which have ever been

read before this Society. The subject of batteries is one we are all interested in—telegraphists of course above all, for they can make no step in their art till they have a battery, whilst those more scientifically disposed, who have no practical connection with telegraphy, must feel an interest in the very able discussion of the theory of the subject which Mr. Sivewright entered upon at the commencement of his paper. It will be obvious that it is quite impossible to attempt to discuss the paper to-night, on account of the time; therefore it will be necessary to remit the question to the next meeting. In the meantime, I think it desirable that we should distribute copies of the paper which has been read to those who desire to have them, in order that those present may refresh their memory, and that those who were absent may attend at the next meeting, and join in what I venture to hope will be a highly-interesting discussion. I need not say there are numberless interesting questions in connection with batteries, many of which, I hope, will be considered and discussed at the next meeting. If there are any members present who cannot attend on the next occasion of our meeting, we shall be happy to hear now any remarks they may have to make.

The following Members were balloted for and declared duly elected :—

As Members :—

Augustus Stroh	. .	London.
James Briggs	. .	Calcutta.

Associates :—

William Marshall	. .	Cheltenham.
J. Donaldson .	. .	Cape of Good Hope.
Robert Hardy	. .	Reading.

The Meeting then adjourned.

The Thirty-fourth Ordinary General Meeting was held on Wednesday, the 24th March, 1875, Mr. LATIMER CLARK, President, in the Chair.

After the transaction of the usual preliminary business,

The PRESIDENT said, We will now commence the discussion on Mr Sivewright's paper on "Batteries and their Employment in Telegraphy," which was read at the last meeting. Before commencing that I will ask the Secretary to read one or two letters we have received from gentlemen interested in the subject, after which I will ask members who have any remarks to make to do so.

The SECRETARY then read the communications, which appear in the Appendix.

The PRESIDENT: I will now ask gentlemen who have any observations to make to commence the discussion. In the first place, I will call upon Mr. Walker.

Mr. CHARLES V. WALKER, F.R.S.:—In obedience, Sir, to your call, I rise to offer a few observations on the subject of the paper that was read at the last meeting. That paper carries us back to bygone days, the days to me with the "London Electrical Society," as Member, Honorary Secretary, and Treasurer, from its foundation on the 10th of June, 1837, to its dissolution on April 22, 1843—to days when Daniell's battery was new to science. It had been given to the scientific world on January 23rd, 1836. (Phil. Trans. 1836, pp. 107-8.) He had originally proposed to call it the "Dissected Battery." One of the early communications brought before the London Electrical Society, October 10, 1838, was on Daniell's "Constant Battery." I took active part in the experiments therein described, in conjunction with Mr. Gassiot, Mr. Sturgeon, and Mr. Mason; and well remember our having thought it a great thing to have mustered 160 (!) cells of Daniell's battery,—the most extensive series that up to the date in question

had been constructed. I had the pleasure of an introduction to Professor Daniell; and met him from time to time till within a few days of his sudden death. He was delivering a short course of lectures in his usual happy style in the laboratory of King's College. Having given his third or fourth lecture (the only one from which I was absent) he left the table to attend a Council meeting of the Royal Society, then held at Somerset House, almost under the same roof as his lecture room. He had scarcely taken his seat at the Council table when his head fell forward, and he was dead. A medical friend of mine who was present at the lecture in question noticed no change in him then. His death was a great shock to men of science. He was a man courteous and kind, especially to the young aspirant, to whom it was a great pleasure to have however small an acquaintance with him.

It is interesting at the present day to read the reports of our very humble experiments in those early days with Daniell's battery. The mathematical laws of current electricity were then known in England only to the very few who were conversant with the German scientific works, among which number we were not. Out of Daniell's battery in some sense Electrotype arose. Professor Daniell himself was among the first to notice that, when the copper deposited in his cells upon the copper plates was stripped off, it bore all the marks and scratches of the plate from which it had been stripped — it was a veritable Electrotype. Taking up the subject of electrotyping some thirty odd years ago, I used in the first instance Daniell's battery; but the cost of its maintenance soon led me to adopt what in principle was Smee's one-fluid battery, in which the hydrogen set free at the negative plate is released by the minute particles of platinum with which the plate—a plate of silver—had been previously coated. Smee's battery dates from the year 1840. (Phil. Mag. vol. xvi. pp. 315–321.) For practical purposes, in the early experiments on Electrotype I substituted copper for silver, and rough electrotype deposit of copper for Smee's fine powder of platinum; the deposit of copper having been made by electrotype in earthenware jars, the inside of which had been first covered with wax and blackleaded. I used—and it

was very useful—such a battery of zinc and roughened copper in making for the late Sir Henry De la Beche the large copper electrotypes, of from 10 to 14 square feet, of the "Triumph of Alexander," which some of you may have seen in the Museum of Practical Geology in Jermyn Street, and where doubtless they may still be seen. Thus early, and notwithstanding my still earlier associations, and for purely economical reasons, I had ceased to use Daniell's battery. For telegraph work on the South Eastern Railway I have for many years substituted platinized carbon or graphite for Smee's platinized silver.

Leclanché Batteries.

But before describing the construction, the cost, and behaviour of this form of battery, I will give the Society the history and the result of my experience with our more recent acquisition, the Leclanché battery, and will then deal with the platinised graphite battery in like form—being familiar with the cost and behaviour of both. I first heard of the Leclanché battery itself on November 28th, 1868, from Mr. R. Sabine; and subsequently of its good conduct from Mr. W. H. Preece, who had some cells in his private house.

It was the very thing wanted at that moment in guards' vans in trains fitted with electric communication; the one-fluid graphite batteries, which had been in use for this purpose for the previous two years, being very inconvenient. On December 18, 1868, I ordered fifteen cells from Paris; and on April 16, 1869, went to Paris and obtained much information from M. Banderali, the friend of the inventor; and on my return, on April 23, ordered 500 cells, of which 250 were for the late Electric Telegraph Company, at the request of Mr. Culley, made on April 21. They all came to hand on May 22, and were the first large consignment that reached this country.

On September 23 I introduced M. Leclanché and M. Banderali to Mr. Scudamore, having previously given them the necessary introduction to Mr. Gray, the manager of the Silvertown Company, with a view to that Company undertaking the agency as well as the

manufacture in England. They did not manufacture them at first as well as they now do. Under all the circumstances, I naturally felt in a measure responsible for their character, and noted any weak points, and by correspondence with the inventor had them corrected. Among other things he told me, as we find him stating in the letter read this evening, that if we use the French porcelain we shall cease to have breakages of the porous pots, which so much troubled us in early days; and he gave me much other useful advice.

You are all doubtless acquainted by this time with the construction and probably the price of the Leclanché cells—2*s.* 3*d.*,—2*s.* 6*d.*,—3*s.* 4*d.*,—4*s.* 7*d.*, according to size. There are several varieties on the table. The Leclanché battery possesses great advantages as a travelling battery. It may be sealed down as tight nearly as a pickle-bottle. They have been used in our guards' vans for six years. The first lot remained for about two years in the vans without having been touched at all; their great feature is their being as good a waiting as a working battery. I have before me a list of Leclanché batteries, which have worked and waited entirely untouched for twenty-nine, twenty-six, twenty-five, twenty-three, eighteen, fifteen, and fourteen months respectively. Table I. (p. 154). Maintenance is the great question with telegraph engineers—the cost per cell per annum. I have gone into this question thoroughly, and am able to lay before the meeting in detail the actual cost of maintenance on the South Eastern Railway. We maintain at work, in round numbers, 3,000 Leclanché and 9,000 platinized graphite cells (the actual numbers are at the bottom of Table II. p. 155); as to wages, I have separated the time spent by the line-men in battery-work from the time devoted to other work, besides attending to the batteries. We have on record all battery stores sent out during the last three-and-a-half years, and all old stores collected in, which I have taken for my data; and have worked out the values very carefully. Most of our Leclanché cells are No. 3 size; but we have also a considerable number of No. 2. Taking them one with the other, the stores for maintaining one cell for a year, omitting decimals, cost 8*d.* The total wages amount to

$7\frac{1}{2}d.$ But here comes in the very difficult question, viz., how to divide this $7\frac{1}{2}d.$ between the time actually devoted to the batteries themselves and the time occupied in travelling to and from the batteries. For instance, say South Eastern batteries at Reading;—it takes a line-man two hours to travel from his central station to Reading, and two hours to travel back, not including waiting for train-time; the batteries themselves occupy him perhaps from half-an-hour to an hour. The conclusion to which I have arrived, as fairly as the division can be made, is that the travelling and waiting time is about six times the working time. So that to maintain a Leclanché cell for a year costs $15\frac{1}{2}d.$, which is made up of $8d.$ for stores, $1\frac{1}{2}d.$ for labour, and $6d.$ for time of travelling and waiting. Table II. (p. 154) gives the items in detail, the weight and value of stores used and the wages.

Platinized Graphite Battery.

I had long held graphite—the corosion from gas retorts—in high esteem. I contributed a hollow cylinder to the Polytechnic Institution some thirty odd years ago; and no doubt it is there still. On April 5th, 1849, I substituted graphite plates for the plain copper of Cooke's Sand-Battery. The first, a 12-plate battery, worked a double needle circuit for two years without having been dismantled. I commenced platinizing graphite plates, and sent out the first battery on October 20th, 1857.

It consists of an earthen jar, a zinc plate, a graphite plate, and a gutta-percha slipper for holding mercury The cost of one cell at present prices is $16\frac{1}{4}d.$ uncharged, and $21\frac{1}{4}d.$ charged. The cost in detail is given in Table III. (p. 155). The charge is sulph. ac. 1, + water 8. There is a cell on the table before you. For connections the top of the graphite plate is covered with electrotype copper, tinned. A full description of the mode of preparing the battery is given in the "Proceedings of the Royal Society," vol. ix. pp. 28-33, February 3, 1859, and still more in detail in "Electrotype Manipulation," 29th edition, August 1859, pp. 42-5; both which works are doubtless in the library recently presented

to the Society by the executors of Sir Francis Ronalds. The speaking circuits and the block system on the South Eastern Railway had been entirely and are now for the most part worked by these batteries. Leclanché's are substituted,—and very valuable they are,—for stations outlying and not easily accessible. One Leclanché about takes the place of two graphites.

When a graphite battery is well made and set up with due care it will do a large amount of telegraph work for a long period, not only without having a fresh charge of solution but without being touched at all. For instance, the batteries set up when the telegraph office at Charing Cross was first opened stood for fifteen months, doing all the through paid and service message work at that busy station without being touched in any way whatever. Table IV. (p. 156) is a list of such batteries working for seventeen, fifteen, fourteen, thirteen, and twelve months entirely untouched. In all forms of battery, liquid being a necessary part, evaporation more or less is always present, and very troublesome it is. The Charing Cross battery referred to was well placed in a dry and cool cellar, and suffered little from evaporation.

It is not all batteries that can be so favourably housed. As a rule they have a rough time of it, and must very often be set up in places totally unfit to receive an instrument on which so much depends, and to maintain it in full efficiency during what one might call the term of its natural life.

The cost of maintaining a graphite battery is small. In Table V. (p. 156) are given figures got out, as were those for the Leclanché's. Per cell per annum for stores is 5*d.*; of this $3\frac{1}{2}d.$ is for mercury, which at present is unfortunately very dear—6*s.* $4\frac{1}{2}d.$ per pound. It used to be about one-third this price, when the stores for a cell would be 3*d.* and the maintenance per annum $10\frac{1}{2}d.$ instead of as now $12\frac{1}{2}d.$ I have taken the wages as divided equally between graphites and Leclanché's *pro rata*, and have subdivided in similar proportions into actual labour and travelling. In Table VI. (p. 157) the relative cost of maintenance of the two batteries is given side by side.

It would be very instructive to have a Table got out in a similar

manner for a Daniell's cell by one of the many Telegraph Engineers who have large experience with this battery.

Mercury is a small item for Leclanché cells—4*d.* per annum for 100; but as it is much more largely used with the zinc-graphite cells it may be as well to tell you that our mode of dealing with the old zincs is to collect them all in, and distil back what mercury remains, obtaining by distillation about one-third the amount of mercury originally given out; that is, if we purchase and send out 30 lbs. of mercury we collect in 10 lbs. by distillation. You will doubtless say, as I do, that we ought to obtain more; but much is lost through carelessness and waste.

Our plan of distilling is this: we have a fire-brick furnace, with a hole nearly a foot square to receive a cast-iron retort, $10\frac{1}{4}$ inches square by $14\frac{1}{2}$ high, on which a lid is tightly wedged, and luted with loam. An iron pipe is tapped into the lid; and under the influence of the fire the mercury soon begins to come out, drop by drop, into a pail of cold water placed beneath the orifice of the pipe to receive it. The charge of the retort is 80 lbs. of old zinc; from which about 7 lbs. of mercury are recovered at a cost of 4*d.* per lb. —a labouring man doing the distillation. Two-thirds of the amount of new zinc originally given out is collected in and sold as old metal; the proceeds of this and of the mercury are taken into account in the figures I give in.

THE PRESIDENT: Can you inform us what is the relative electromotive force of the batteries you have mentioned, and also the resistance?

MR. WALKER: The electromotive force of the Daniell being taken as 1, Leclanché states his to be 1·382. The resistance of No. 3 size he gives as about 4 ohms. The electromotive force of the plat-graphite cell may be taken to be the same as Smee's, which would be about the same as Daniell's; and the resistance like Smee's, about 1 ohm. (Vide Latimer Clark on Electric Measurement, pp. 108-191.)

	Elec. F.	Resist.
Daniell . . .	1·0	5 to 15 ohms.
Graphite (qy?) . .	1·0 to 0·5	1 ,,
Leclanché . . .	1·382	4 ,,

Mr. W. H. Preece: Will you inform us whether the 6*d.* charged for the travelling in the maintenance of the Leclanché battery is made up by the men's time spent in travelling or whether it includes the value of his fare?

Mr. Walker: Of the 7½*d.* wages per cell paid to the line-men; 6*d.* is for the time occupied in travelling and waiting. It does not include railway fares. I may here add that tools, shops, carriage, and superintendence are not included in the data from which the Tables are constructed.

TABLE I.—Leclanché's Untouched.

Reigate	29 months
Chislehurst, goods	26 "
Charlton	25 "
Sand Street	23 "
New Cross	18 "
New Beckenham	18 "
North End	15 "
Higham	14 "
Beckenham Junction	14 "

TABLE II.—Leclanché Batteries.

Maintenance—per cell per annum.

	Oz.	*d.*
Mercury	0·008	0·038
Zinc	0·551	0·568
Sal-ammoniac	4·347	1·410
Charged porous		4·986
Complete cells		0·297
Glass cells		0·873

	d.
Stores	8·172
Labour	1·291
Travelling	6·036
Total	15·499

1874. Dec. 31.	Graphites in use	9242	
	Leclanché's	3092	
	Total		12334

TABLE III.—PLATINIZED-GRAPHITE BATTERY.

Prime cost of 1 cell, 6-in. × 2-in.

	d.	d.
Pint jar	2·375	
Graphite 6-in. × 2-in.	3·435	
Zinc	1·911	
Slipper	2·833	
Copper band	·882	
Copper rivets	·222	
		11·658
For Electrotyping :—Zinc	·15	
,, ,, :—Copper	·32	
,, ,, :—Sulph. of copper	·09	
,, ,, :—Sulph. and muriatic acid	·10	
		·66
For Platinizing :—Chl. Plat.		·09
,, Soldering :—Solder		·07
,, Amalgamating :—Mercury		1·20
		13·678
Labour		2·582
Uncharged		16·260
The charge for one cell, mercury	4·781	
Sulph. acid	0·230	
		5·011
Charged		21·271

A $7\frac{1}{2}$-in. × 3-in. cell costs 31·933*d*.

TABLE IV.—GRAPHITES UNTOUCHED.

Maidstone	17 months
Etchingham	17 ,,
Charing Cross . . .	15 ,,
Bopeep	15 ,,
Hastings	15 ,,
Battle	14 ,,
Tunbridge Wells . . .	14 ,,
Sevenoaks	13 ,,
Smeethe	13 ,,
St. Leonards . . .	12 ,,
Maidstone	12 ,,

TABLE V.—PLATINIZED-GRAPHITE BATTERY.

Maintenance—per cell per annum.

	oz.	d.	d.
Mercury . .	0·749	—	3·62
Zinc . . .	1·888	0·94	
Copper . . .	0·051	0·12	
Sulph. acid . .	6·876	0·36	
Chloride of platinum . .		0·01	
			1·43
Stores			5·050
Labour			1·291
Travelling			6·036
Total . . .			12·377

NOTE.—One-third of the mercury and two-thirds of the old zinc are recovered by distillation—8lbs. gives 7 zinc + 1 mercury. Two-thirds of old copper are collected in.

TABLE VI.—Comparative Cost.

Maintenance—per cell per annum.

	Leclanché.	Graphite.
	d.	d.
Mercury	0·038	3·62
Zinc	0·568	0·94
Copper	—	0·12
Sulph. acid	—	0·36
Chloride of platinum . .	—	0·01
Sal-ammoniac . . .	1·410	
Charged porous . . .	4·986	
Complete cells . . .	0·297	
Glass cells . . .	0·873	
	8·172	5·050
Labour	1·291	1·291
Travelling	6·036	6·036
Total . . .	15·499	12·377

Mr. Hawkins: Mr. Sivewright having at the last meeting so thoroughly brought to the notice of the members the many various kinds of batteries that have been employed within the last twenty years, it needs no comment from me, except on the Leclanché battery, on which if I may be permitted I would like to make a few remarks.

I remember at the latter end of 1868 a Leclanché element was brought to my notice on which I made many experiments. I found it to be a battery of high tension, low internal resistance, and great quantity. It remained very clear, constant, and of great quantity, but the weakness was in want of constancy when worked through a low resistance. This cell after fourteen months (during which time it was in no way renewed or refreshed) gave within ten per cent. of its original force and quantity, the internal resistance remaining nearly the same. I ought perhaps here to observe that this cell was hermetically sealed to prevent

evaporation; the only observable difference in it was that the lead top began to show signs of a slight decomposition, but was passed by without being looked upon very seriously. This cell was tested with others, and the results obtained month after month were very slightly different in electromotive force, but the internal resistance kept gradually increasing, and to ascertain the cause I determined to break up the cell. I found on examination that the lead top was reduced to white lead all over that part of the carbon which was in contact with the lead connections; this was doubtlessly caused by the creeping up of the hydrochlorate of ammonia by the porosity of the carbon plate. I next made a cell on the same principle as the first-named, following everything as nearly as possible, and to prevent creeping I took special care that the end of the carbon that was to receive the lead top was carefully pickled in hot paraffin wax for one hour at a temperature of 230° Fah. This would appear at first an unwise step as it would be thought to produce inferior conducting results, but such was not the case; and furthermore to improve the contact of the lead top with the carbon plate two holes of $\frac{1}{4}$ in. diameter were drilled through the end of the carbon, and the lead being poured into a mould around the carbon and brass screw (for the connections) in a molten state, filled up the holes in the carbon plate, and by the contraction while cooling acted as two rivets and so improved the contact. This as an experiment took some time to prove, as time was requisite to develope that weakness which followed in the first cell.

I remember the Post Office authorities procuring a quantity of Leclanché cells in or about the early part of 1870, and those batteries were, if I mistake not, specially stipulated to be of the size adopted by the Department, and that nothing but Wedgwood's red porous pots were to be employed, and doubtless this was so stipulated from the fact of the Department having for some years used none other than Wedgwood's, or that they had proved from experiments that Wedgwood's ware was most suited to the batteries they were then using. I believe, as far as I can learn, the cells adopted by the Department were of small size, viz., porous pots about 3 in. deep and $1\frac{3}{4}$ in. diameter; those on the table

before me appear to be the identical size adopted, and apparently have failed in the same way as the first cell named by me, by the lead top being reduced to white lead by the creeping up of the salts of ammonia, and in some, as Mr. Sivewright justly observed, the porous pots "flaked" and in others burst. The cause of this I am not quite clear upon, as many even of the red pots remained sound in the same battery while others were burst and flaked. I verily believe from my experience that the red pots are the least suitable, being very soft and porous—and that the great cause of bursting is the crystallization of sal-ammoniac within the porous pot in a solid form. This crystallization is found more when batteries are standing with an insufficient quantity of liquor. I have not on any occasion seen a burst or flaked pot that had been hermetically sealed so as to prevent evaporation. The porous pots used by Leclanché in Paris, are, I believe, of a very different substance from those procured in England, being much harder and less liable to flake. In the cells made by the sole manufacturers and vendors of this battery in England (the India Rubber Company) only the very best French porous pots are employed, hence bursting or flaking is of rare occurrence. Flat plates should be of the same kind of ware, but I believe those employed in the converted trough battery are all English ware, hence the cause of flaking in this particular form.

I observe before me a battery made to effectually prevent this evil, being constructed as follows: The box contains ten cells, the outer cell being of ebonite, the porous diaphragm is cardboard, one-eighth of an inch in thickness, with a piece of thin ebonite (perforated) at the front of the cardboard to permit porosity and to act as a wall in a curved form from corner to corner, thus fitting firmly in the cell and made watertight by pouring hot paraffin wax around the sides and bottom. The usual mixtures employed in the Leclanché patent are then placed in one part and sealed, while the other part is left open (being small) with the zinc, so that the height of the liquor may be observed. This kind of porous diaphragm has shown no signs of failure, I having had some under test for over twelve months. Again referring to the lead top, the second cell I made

experimentally to prevent the decomposition showed no signs of failing, and, in conclusion, I can but say I have not seen a lead top fail for certainly over two years, excepting those made prior to that earlier date, when the experience was very limited. I will also remark an experiment on a Leclanché element worked in a continuous closed circuit. I placed in circuit with a No. 2 Leclanché cell a resistance of 200 ohms, last December, and tested it periodically; my test on Saturday last showed the electromotive force to be within ten per cent. of its original force, and after one minute free it increased two per cent. and at the end of ten minutes it was perfectly steady and then within 4 per cent. of its former force. I also at the same time placed another cell of the same size in circuit with 1,000 ohms, and it was subjected to tests exactly the same as the previous one on Saturday last; I found the electromotive force to be within one per cent. of what it originally was, and after one minute free no perceptible difference in force was apparent. From a series of experiments I find that batteries fall in potential after one minute's short circuit as follows:—

No. 1 Daniell unit cell . .	=	0%
Marié Davy	=	1·86%
Grove	=	0·51%
Bunsen	=	6·82%
Meidinger Sulphate . .	=	2·89%
Gravity ,, ...	=	6·71%
Leclanché No. 2 . . .	=	20·90%

Therefore the fact is clearly established that the Leclanché in its present form is quite unfit for a continuous or local circuit of 21 ohms, but by interposing resistance it can be used with good results. Leclanché will be found almost constant if worked on a busy line where the resistance in circuit is equal to 100 ohms per cell, and I believe if every telegraphic circuit were so arranged that no Leclanché battery should be placed in a line that had less than 100 ohms resistance to every cell applied, in many instances a great saving and a more satisfactory result would be arrived at. Many imitations and infringements of Leclanché's patent battery

have from time to time been brought to my notice, in which I have invariably found very inferior material and manufacture; from batteries so manufactured inferior results must inevitably follow. I am of opinion that too much care cannot be bestowed on the manufacture of such a battery, and that it needs nothing less than the very best materials to ensure those practical results that are now obtained, and that cracked porous pots and destruction of the lead tops will be things of the past.

Mr. W. H. PREECE: Can you kindly inform us what are the relative electromotive forces and internal resistances of the different forms of Leclanché batteries?

Mr. HAWKINS: The resistance of the battery No. 1 was about 2 ohms, of No. 2 about 2·5 ohms, and of No. 3 about 2·75 ohms. The electromotive force of each of the three sizes is 1·5 as compared with a Daniell.

Mr. ALFRED BENNETT: It is a mistake to suppose that no local action whatever takes place in an inactive Leclanché battery. If a cell is left idle from day to day, and the zinc carefully weighed after being freed from crystals, it will be found to lose from three to four grains every twenty-four hours.

Some time ago I undertook an extended series of experiments with the object of finding a battery as cheap, simple, and effective as that of Leclanché. After trying a great number of combinations I came to the conclusion that such a battery was obtained by using a plate of carbon packed in a porous cell, with pieces of the same substance as a negative element, with zinc and a solution of hydrate of potassium as a positive. No liquid should be placed with the negative, as it is essential that the air should have free access to the cell; but sufficient water speedily percolates through the porous diaphragm to render the carbon as damp as is desirable.

When the circuit is completed the oxygen, both of the water and potash, is liberated at the positive pole and forms oxide of zinc. The potassium and hydrogen are set free at the negative plate, where the potassium is instantly re-converted into hydrate on the surface of the damp carbon. The oxygen thus attracted from the atmosphere leaves azotic gas, which combines with the hydrogen

and forms ammonia. The battery in time thus becomes a species of Leclanché.

The electromotive force of this combination is about the same as the Leclanché—if anything rather higher; but as it does not fall nearly so rapidly when working through a moderate resistance its effective strength under such circumstances is much greater. It is perfectly constant through 70 or 80 ohms, whilst the Leclanché fails to keep up its original strength for any length of time when working through a less resistance than 200 ohms. The re-conversion of the evolved potassium to hydrate causes the battery to last a long time without a fresh supply of material. It is a clean battery, and unless a very strong solution—which is not necessary—is used there is little or no local action. It is now in use in various parts of the United Kingdom, and I have received very gratifying reports of its performance. I may mention that hydrate of sodium is quite as effective as potash when first made up, but owing to its more rapid absorption of carbonic acid it changes to carbonate sooner, and, besides, makes a dirty battery. I was some time in discovering these defects of the sodium salt, and as it is the cheaper of the two all the first batteries on this principle were made up with it, but its use has now been discontinued in favour of the potash.

I lately resumed my experiments with the hope of obtaining a still more constant battery. After innumerable failures I am happy to say that I have succeeded, and have also lighted upon one or two facts which I think will be of interest to the Society. The details of my experiments are almost too voluminous to be communicated during a discussion, and I therefore propose, subject to the approbation of the Committee, to lay them before you in the form of a paper at an early date, when I hope to have the honour of submitting to your notice a single fluid battery with the potential of the Leclanché and almost the constancy of the Daniell.

Dr. John Hall Gladstone, F.R.S.: I had not the advantage of being here on the last occasion, but I have been afforded the opportunity of reading the paper by Mr. Sivewright, and have had great pleasure in seeing the way in which he treated the subject. His paper opens up a number of questions which one would like

to go into. Some of these are of a purely theoretical order, but there are other researches which could not fail to be productive of good in the hands of the Members of this Society who have time to give to such matters. One occurs to me. Mr. Sivewright remarked that it is a point of interest whether osmose through the porous cell of a Daniell's battery is accelerated or retarded when the battery is in action. One would think that was a matter which might be determined easily by experiment; but it is not so simple as it looks, because we have the contents of the cell continually changing their character. We have the osmose of several different bodies at work, and not merely osmose but mechanical movement of the liquids to a considerable extent. Any one who examines the matter will find that in a Daniell's cell there is a heaping-up of sulphate of zinc, and consequently a current downwards, against the zinc, but where the copper is deposited the sulphuric acid element passes away from the copper in the other direction, and an attenuated solution is produced, so that there is consequently an upward current; therefore in the two parts of a Daniell's cell there are two circulations going on at the same time. These are mechanical and must to a certain extent modify the osmose.

Another point is this. We have heard a great deal about the Leclanché battery. I am not sure that we quite understand the chemistry of that yet. My own study of the subject leads me to results different from the formula which Mr. Sivewright has given, which looks pretty upon paper, though I think he does not quite believe it is borne out in point of fact. Hearing Mr. Walker's remarks upon Professor Daniell one could wish that there were many such men in the present day, who would investigate these questions. There has of late been a prodigious development in the whole matter of batteries through the requirements of electric telegraphy, but the investigation has been rather in the direction of the electrical and mechanical than the chemical part of the question, the work actually taking place in the battery. There is here a large field for numbers who like to enter into it.

I do not believe we have yet got the ideal battery. It would be possible to try a great number of combinations with prospect of success.

It is true batteries are cheap, but one can imagine they might be cheaper. If I may give my own experience I know of a form of battery cheaper than any of those now before us. The oxygen of the air is the only liquid element used up in it. It was first made with silver and copper, but that will not do in actual practice. Since then it has been produced with copper and zinc, and an ordinary aerated solution of chloride of zinc. The oxygen of the air dissolved in the liquid causes decomposition, and oxide of zinc is deposited on the copper, while the zinc dissolves away, and all the phenomonen of a galvanic current are produced. This is theoretically the cheapest of all batteries. The electromotive force is high, and the internal resistance is small, but there is one fatal quality about it, viz. it is by no means constant. At first it is very active, because of the oxygen dissolved in the liquid; but that soon begins to be used up, and the force runs down rapidly. The whole subject is one of great interest from a scientific as well as from a practical point of view. It is astonishing the large number of forms of batteries we have, but no doubt, from the suggestions which may be made in the course of this discussion or from the thoughts which may occur to our minds, something still better may be produced than we yet possess.

The PRESIDENT: Can you give us an idea of the form of that air battery?

Dr. GLADSTONE: Mr. Tribe and I described it before the Royal Society, and it is published in their Proceedings. One peculiar mechanical arrangement is necessary. It is necessary to bring the copper as near as possible to the surface exposed to the air. We arrange that the plate should lie on the surface of the liquid. In using the silver battery we had a porous tray of silver, carrying crystal of deposited silver. The force soon runs down; it gets to a steady action afterwards, but that is very small.

Mr. ALEX. ADAMS: It was mentioned in the first letter read this evening that oxidisation does not take place with carbon. It appears that some kind of action does take place. I obtained some comparatively pure carbon, cleaned it with acid, and afterwards brought it to, and baked it at, a white heat. The carbon plate fortunately

split longitudinally whilst in the fire, and after being washed was baked again. I placed the two pieces of carbon, of similar size, in condensed water in a glass, and tested them for current through a very sensitive reflecting galvanometer. The deflection produced was about 12 *inches* of scale, which during thirty hours gradually fell to zero, where it remained. Lifting the carbons from the liquid, and replacing them, produced a deflection of about three inches. This led on to experiments with a Daniell's cell, the results of which tend to shown that we have little knowledge as to its true internal *modus operandi.* I had forty cells of the ordinary Daniell form, and marked one of the number for experiment. After the forty cells had been joined on short circuit by means of switches, the experimental cell was tested for its electromotive force. Sixty cells were then opposed to the forty for one minute, and upon again testing the one cell I found its value had increased. I then obtained a glass jar, and placed within it a piece of the carbon already referred to, with water. After the forty cells had worked on short circuit for some time the zinc was lifted from the experimental cell into the glass jar opposite the carbon, and the deflection noted. Returning the zinc to its cell the sixty cells were opposed to the forty, and the metal again lifted into the glass and tested, when the deflection exactly equalled that of the previous test, intimating that no change had occurred upon the zinc. After placing a cell in opposition to the zinc-carbon water-cell, and again shunting the latter through the galvanometer, I found that the electromotive force had increased by about ten per cent., and this result, which was several times verified, appears to explain in some measure the wide difference between the motive capacity and the chemical action of a cell. It seems that the force exerted at the first instant of time alters the condition of the negative plate, and thereby instantaneously reduces the value of succeeding forces. I also find that by allowing the opposing cell to act for some minutes a much higher force is afterwards obtained, but that it gradually subsides. I cannot satisfactorily account for the large increase of force just mentioned.

With regard to the Daniell battery, but little has been said in

connection with the form of cell commonly in use. We have what is called the ordinary Daniell, but it appears to me that the amount of metallic surface is too small. There are, as we know, considerable climatic differences upon our globe, notwithstanding which the cell used is more or less generalised. For many foreign lines which are comparatively constant the present plates are suitable, but I think them unequal to the varied emergencies of a climate like our own, where lines hardly remain constant for half-an-hour. To get over the failure of a circuit in consequence of weather leakage, we add cells in series, thereby largely increasing the battery-resistance, whilst the line-resistance has fallen. Surely this is not in accordance with theory. By increasing the size of the plates to (say) half as large again, we reduce the resistance of the battery to about 4·8, its original value; the effects of bad weather upon the line would not be so quickly felt, and upon eventually adding cells the battery-resistance would increase less rapidly. We have heard about the gravity-cell, and of the Daniell's cell, with its porous pots. The Siemens' modification effects a combination of the two, and from what I have seen abroad this cell works remarkably well, the only fault being its high internal resistance as compared with the Daniell. For constancy I do not know a better form of battery, but, unless its internal resistance be somewhat modified, I doubt if the Siemens' would be so advantageous for this country: it is one of the cleanest double-liquid batteries extant. One remark more. I have found by experience that upon asking a person to test a battery he will put it on short circuit through a galvanometer of low resistance, and if the deflection is large will pronounce it to be in good condition; but it does not appear generally known that the galvanometer should be left in circuit with the battery for five or ten minutes, and the fall of the needle noted, and that the fall divided by the time gives the desired replication. This is not generally practised, and consequently exhausted batteries often cause indifferent working, without the slightest suspicion that the fault lies in them.

Mr. E. Graves: There is an allusion in the paper to some experiments made on the Leclanché battery with a solution of common

salt instead of sal-ammoniac, and although I cannot contradict the conclusion arrived at, that those experiments generally were failures, yet I have some instances of success which may be worth mentioning. In January last year a rough battery of 10 cells was fixed at Enstone, near Oxford, on a circuit of 40 miles in length of No. 8 wire, and with five single needles in circuit. I do not know what its exact resistance is. The office was one in which the work done is little, not amounting to more than four or five messages per day. The battery had water added in October last, and was looked at, but not touched; in January it was then working well. Another instance was at Chester. I have got the figures of comparison between this battery and a Leclanché fixed in the same locality, charged in the ordinary way with sal-ammoniac, whilst the former was charged with common salt. I will read you an extract from the report of Mr. Edwards, the Superintendent of Post Office Telegraphs at Chester, dated March 23, 1875.

He says—"A trough Leclanché charged with sal-ammoniac was joined up at one of the Shropshire Union Canal stations in September 1873. Since that date it has been four times refreshed with water only. Once; water and sal-ammoniac have both been added. A day or two since I examined the battery carefully, and found it in almost as good a condition as when first joined up. There was no scaling or splitting of the porous plates observable. The zinc plates were fairly clean and as good as new. I put the battery constantly in circuit with a quantity detector for one hour and a half, with the following results. When first placed in circuit the battery gave 28°. In five minutes it dropped to 24°. In twelve minutes to 15°. In thirty minutes to 13°. In forty minutes to 12°; and thence onward to end of the hour-and-a-half it remained stationary at the last-mentioned figures.

"Another trough Leclanché, on the Chester and Birkenhead circuit, was charged with salt in lieu of sal-ammoniac in November 1873. Since that date water only has been thrice added. It has been moderately worked. An average of forty messages per day passed over the circuit throughout the whole period, but on some days there were double that number. I tested it in the same

way as the other, keeping a quantity detector constantly in circuit with the battery for an hour-and-a-half. Results as follows: When first joined up 24°. In five minutes 20°. In fifteen minutes 16°. In thirty-seven minutes 15°. In seventy-two minutes 14°, and at the end of the test 14°.

"These figures show the salt battery to be absolutely more constant than that charged with sal-ammoniac, even allowing for the former being put to work two months later than the latter. In the salt battery I found the porous plates perfect, as also the carbon and zinc."

With reference to the causes of failure of the Leclanché, I believe them to have been mainly due to the defective character of the porous plates. I think the experience I have had confirms the view that such failure arises from no inherent defect either chemically or mechanically in the arrangement of the battery, but rather from the imperfect material employed for the porous divisions. It happens that I have a larger proportion of Leclanché batteries of the forms which have been introduced during the last two or three years than of the earlier kind, and I find that failures with me are very much fewer than those which have taken place in parts of the country where a large number of the earlier forms have been employed. I have made inquiries, and I find nothing which admits of the question being disposed of by a simple reference to the Wedgwood-ware cells that Mr. Hawkins states the Post Office to have specified for. Railway Companies have not so specified, but whilst my brother, who is Superintendent of the North Eastern Company's Telegraphs, informs me that he has 5,000 cells of Leclanché's battery and has had very few cases of failure, the Superintendent of the Lancashire and Yorkshire Railway Telegraphs, using the same form of battery, states that failures are frequent from the splitting or flaking of the porous plates.

With reference to the formation of white-lead between the carbon plate and the lead cap, I think it arises from the varying conditions of manufacture. One lot of 400 cells of the Leclanché form supplied to the Post Office at Liverpool some eighteen months ago failed to such an extent that in less than four months 250 out of 400 were

condemned. On the other hand, for the whole time subsequent to the date I speak of, the failures have been very rare. The economy in maintenance arising from the use of the Leclanché battery seems to me to be perhaps slightly overrated. There is a decided economy in consumption of material; but, when you take the practical cost of maintaining batteries, the cost of material as a rule forms but a small proportion of the actual total outlay involved by the occupation of men's time, allowance for expenses, and payment of fares. It is true that a man may at long intervals visit an isolated office and find the battery requires nothing to be done to it; but, from the tendency of the Leclanché when it does go to go without giving warning, it is impossible to allow offices of the class I speak of to remain unvisited for an excessively long time, consequently the most material outlay occurs whether there is a consumption of material or not, and I would therefore point out that the economy is not so great as at first sight appears.

The collected experiences of the use of the Leclanché battery present a mass of contradictions. It is the general experience that the great fault of the battery is its want of constancy, and its consequent unfitness for locals or for heavy circuits. I have exceptional record, however, of cases of considerable constancy under circumstances of much trial. These were on two circuits, one from Walsall to Birmingham and the other an instrument at Walsall, intermediate between Birmingham and Stafford; the shorter journey was of about ten miles and the longer about thirty. On each of them a 10-cell Leclanché battery was fixed: one on the shorter wire worked for three years, from the commencement of 1872 till the commencement of the present year. Sal-ammoniac was added several times and water frequently, and one or two special cells were changed. The battery on the other circuit lasted for the same time, but the work was less hard and the line-resistance was greater. By way of bringing the battery to exhaustion point, in January of this year the two sets were connected together and joined as one to the short circuit from Walsall to Birmingham, which had been duplexed. For a short time they worked well and gave at first 40° quantity from the full set, but in a

fortnight it fell to about 5°; after two days' rest they tested as well as ever.

In dealing with the practical maintenance of the Leclanché battery one circumstance has not been touched upon, that is the liability of the connecting wire to fracture if it is rudely handled—the varnish coatings appear to yield after the lapse of a certain time. Chatterton's compound has proved the most successful covering. It is found advantageous to put on two coats of the compound, letting the first dry before the second is applied, and making it thicker towards the lead cap than in the middle.

I do not think I can recall any other features which have not been already fully dealt with. Mr. Sivewright's paper on the whole completely confirms the experience I have obtained. This paper appears to me to be an admirable *resumé* of the general experience derived from the practical use of the batteries most generally employed.

Mr. Higgins: Mr. Sivewright mentions the Groves' battery as having had its day, and being about to be numbered with the experiences of the past, the carbon battery; and states that for general telegraphic purposes they are fast making way for one of the many forms of the Daniell's battery. In the case of the Exchange Telegraph Company I am sorry to be obliged to say that the carbon batteries cannot make way for any form of Daniell's battery, partly on account of the extra space required and partly on account of the great cost of Daniell's capable of producing the amount of force we require to work our automatic instruments. The battery is as described by Mr. Sivewright, except that the solution we employ for the carbon cell is much more concentrated. To make it, 5 parts of bichromate of potash are dissolved in 44 parts water, as far as possible, and 15 parts sulphuric acid added slowly through a syphon while the solution is being stirred, to effect the solution of the remainder of the bichromate; organic matter must be carefully excluded from this solution, especially while warm, as it is readily deoxidised by straws, paper, wood, and other combustibles. The density should be $1\frac{1}{4}$ of water. For the outer or zinc cell, a solution of 1 part sulphuric

acid in 32 parts water is employed. The surface of the zinc facing the porous pot is 25 square inches, the resistance $\frac{3}{10}$ ohm per cell, the potential 2 volts. In practice this is reduced to 1·8 after a quarter of an hour's work, and in calculating the power necessary for a line the latter potential is taken. This battery, though the best for our use, is a most inconvenient one. In winter, if the solutions are permitted to become very cold, its electromotive force is increased, but the battery is much more rapidly exhausted. If the circuit be closed long enough to cause the bichromate solution to turn slightly green it polarises very rapidly, and should it turn very green its removal and replacement by fresh is quite useless to restore the battery to proper working condition. The carbon plates become impregnated to such an extent with hydrogen that only soaking for some time in fresh solution, or washing and drying in the air, will remove it. New plates, or those which have been long exposed to the air, act with great energy for a short time after being set up, an arc of about three-quarters of an inch between copper wires being obtainable with 50 cells. The cost of mercury is almost equal to that of bichromate. It appears that the purity of the zinc has more to do with its regular dissolution than the manner of fashioning it. Rolled plates dissolve until very thin without holes. Spongy or cellular parts of the plates cast in closed moulds are always eaten away in an undue manner in spite of the amalgamation. The most remarkable instance of rapid decomposition of plates which I have seen occurred when a dark powder in patches was found on some plates; the affected parts were rendered invisible while immersed by a dense milk-white stream of minute bubbles which ascended from them. This powder was examined by Dr. Muirhead, and found to consist of carbon with a small quantity of chromium. Holes two inches in diameter were found in plates three-eighths of an inch thick after the plates had been twenty-four hours in the solution. The batteries are required to be of such low resistance compared with the lines they are employed on as to furnish a current of ·5 Webers on two lines of equal resistance simultaneously, and also to give about the same on either line separately.

The quantity of current passing to earth by our earth-wires is so great, that, if a common quantity detector be used to form a derived circuit across two or three inches of any of them, a considerable quantity of current will pass, as indicated by the deflection.

The tray batteries of Sir William Thomson are, as far as I know, the only ones which could replace those we use, but 3,000 at least would be required, therefore the cost would be much in excess of the bichromate, both of installation and maintenance.

Batteries.

In searching for some generator of electricity to replace these troublesome and expensive bichromate batteries, we have tried various kinds, a number of which are quite unworthy of notice. But some present good features, and though perhaps well known it may be interesting to have an account of their capabilities and performances. Some modified Leclanché batteries, manufactured by the Société Générale de l'Electricité of Paris, in a very compact form, with canvass receptacles for the positive pole, offering an internal resistance of 2·6 ohms, are very good for intermittent working where much quantity is required. The pores of the upper portion of the carbon prism are filled with wax to prevent the chloride of ammonium solution attacking the brass-binding screw used for connection. Two such cells are sufficient for a sounder or ink-writer local. Of gravity batteries, we have tried the form known as Lockwood's, with very good results. Three cells charged with 5 lbs. sulphate of copper, 2 lbs. $1\frac{3}{4}$ oz. zinc, and 6 ozs. sulphate of zinc, were put with three other gravity cells, on a closed circuit, with one ink-writer and a sounder; total resistance about 60 ohms. On the 3rd of March, 1874, they were still working, but somewhat diminished in force, owing to the greater distance of the top copper spiral from the sulphate of copper. Internal resistance about 6 ohms per cell at first. Nothing has been done to these cells but to add water to replace that lost by evaporation. The other three cells were put on the same circuit on January 27th, 1874, charged with $3\frac{1}{2}$ lbs. sulphate of copper per cell. In this form the zinc was supported by pieces of wood attached to the copper plate, and as

the sulphate became consumed the zinc and copper plates sank lower. Loose-fitting wooden covers were put on to prevent evaporation. The current appeared to be always of the same force from this battery, and nothing whatever was done to it until July 14th, 1874, when it was taken down, because every trace of sulphate of copper had disappeared from two of the cells. During the whole of this period the circuit was closed day and night, except when messages were being sent. Finding that the diffusion of the sulphate of copper was too rapid for circuits of high resistance and intermittent working, I prepared two cells, in which the copper plates are placed above diaphragms of felt, through which the supply of sulphate of copper solution must pass. One of these has been in connection with ten telegraphic Daniell's, working on a circuit of 100 ohms resistance, closed about seven hours a-day since March 16, 1874. A little more than half the sulphate of copper has disappeared, but no copper has been precipitated on the zinc. The other was charged and connected to a trembler-bell of 6 ohms resistance on the 22nd May, 1874; the trembler is still working. Upon circuits of high resistance, like those described by Mr. Sivewright, these batteries could be arranged to work for about two years without being touched.

Some chromic and sulphuric acids compound was tried in place of the bichromate solution made by us, and found to give a slightly better electromotive force; but its resistance was too great, and when diminished by adding sulphuric acid the depolarising power was insufficient for half-an-hour's continuous work. We have tried Gramme's machine, but the induced current in the bobbin at the time the circuit is closed is so great as to materially shorten the signals; therefore it cannot be used.

Referring to one portion of Mr. Sivewright's paper, I think the very rapid exhaustion of the Leclanché battery mentioned there must have been due to its having been caused to work through a much lower resistance than its own. Three of the smallest Leclanché cells are sufficient to work one of our embossing Morse instruments, and make a very loud sound. To prevent scaling, a glazed porcelain division, with a great number of per-

forations, could be used instead of the porous pot; if the holes were small enough to prevent the manganese falling out, this would answer very well and not scale.

The following Candidates were balloted for and declared duly elected:—

Adolphe Lindeman.
Lieut. C. F. Beresford, R.E.
Lieut. Macgregor Greer, R.E.

The Meeting and discussion were then adjourned.

The Thirty-fifth Ordinary General Meeting was held on Wednesday, April 14th, 1875, Mr. LATIMER CLARK, President, in the Chair.

The preliminary business having been transacted, the President, in terms of deep regret, announced the decease of Mr. Carl Becker, a valued member of the Society, who was known to many present, and was highly esteemed both for his personal character and his great scientific knowledge, and from whose researches they had received great assistance in the manufacture of various electrical instruments. His loss would be deeply felt, and he was sure it would be regretted by all who knew him.

At the last meeting we commenced the discussion of Mr. Sivewright's paper on batteries. Since then a paper on the same subject has been sent in by Mr. Alfred Bennett, and I think it is desirable it should be read now, after which we can discuss the two papers together.

The following paper was then read:

NEW FORMS OF GALVANIC BATTERIES.

By ALFRED BENNETT.

In all galvanic combinations water is decomposed when the circuit is completed. The oxygen is evolved at the positive, and the hydrogen at the negative, pole. The latter gas being electro-positive to zinc, it is necessary in all constant batteries to provide for its neutralisation immediately upon evolution, or the negative plate will become covered with hydrogen and a current be set up in opposition to the primary one. Consequently the effective current from the battery will only be equal to the difference between the strength of the primary and secondary currents.

In the Daniell battery the hydrogen is effectually got rid of by the decomposition of the sulphate of copper contained in the solution surrounding the negative plate.

In the Leclanché the difficulty is partially met by causing per-

oxide of manganese to part with a portion of its oxygen with which the hydrogen combines. The quantity of oxygen given off is, however, insufficient, and the negative plate consequently becomes polarised when the battery is made to work through a moderate resistance.

The only other form of constant battery not made up with strong acids, the use of which is, for many reasons, inadmissible, is that of Marié Davy. The action, which resembles that of the Daniell, is to precipitate mercury from a solution of the bisulphate of that metal by which the carbon plate is surrounded. It is a very good battery for long lines, but, like the Leclanché, fails to keep up its potential when put to hard work. The great cost of the mercury-salt has, however, been the chief obstacle to its use for telegraphic purposes.

It occurred to me that it would be easier to abstract oxygen from the atmosphere and allow the hydrogen to combine with the nitrogen thus liberated than to cause oxygen to be given off from a solid substance, as peroxide of manganese. The great affinity of the metals potassium and sodium for oxygen led me to commence my experiments with them. I soon found that when the three elements, oxygen, hydrogen, and potassium, or sodium only, were present, as in a solution of the protoxides or hydrates of those metals, very good results were obtained; but the presence of a fourth element, as carbon or chlorine, greatly reduced the efficiency of the cell. This form of battery, which consisted of a carbon-plate packed in a porous cell, with pieces of broken carbon as a negative element, and zinc, with a solution of hydrate of potassium, as a positive, continued perfectly constant through 80 ohms, the constant point of the Leclanché being about 200 ohms.

It being evident that the abstraction of oxygen thus effected was insufficient to constitute a perfectly constant battery, I sought for some means of increasing it to the required degree. Phosphorus, placed at the bottom of the porous cell, and covered with broken carbon, answers perfectly; but being extremely dangerous stuff to handle, and further possessing the inconvenient knack of igniting spontaneously and setting the whole battery on fire, it is, for all practical purposes, useless.

I next tried pyrogallate of potash, which possesses the faculty of extracting oxygen from the atmosphere very rapidly. I placed a little pyrogallic acid at the bottom of the porous cell, which was then fitted up with broken carbon. The solution of potash, percolating very slowly from the positive portion of the battery, gradually changes the pyrogallic acid into pyrogallate of potash, which attracts oxygen in sufficient quantity to render the combination perfectly constant, even on short circuit. Pyrogallate of soda is just as efficient; but pyrogallate of ammonia is useless. I tried it with the hopes of increasing the constancy of the Leclanché.

Nitrate of cobalt, when diluted with a solution of potash, is decomposed and becomes hydrate of cobalt, a peculiar property of which is its power of absorbing oxygen from the atmosphere. I took advantage of this fact by substituting a small quantity of nitrate of cobalt for the pyrogallic acid in the combination just described. The potash very gradually reduces the nitrate to the requisite condition, and until this has absorbed as much oxygen as it is capable of taking the battery will remain perfectly constant on short circuit. As the idea of using nitrate of cobalt only occurred to me about three weeks ago, I am unable to say how long it takes to effect this; but I have had a cell working night and day since then, sometimes on short circuit, sometimes through 10 ohms, but mostly through 5 ohms, and it is now as good as ever.

A mixture of sulphur and iron filings or turnings in the same position is always effective, keeping the strength of the battery constant under the most severe work. By absorbing oxygen the sulphur becomes sulphuric acid and the iron protoxide of iron. These combining form protosulphate of iron, which is decomposed by the potash. I have not had this combination on trial sufficiently long to be able to speak with certanty as to its merits, but as the iron seems to get through to the zinc after the cell has been some time at work I am rather dubious as to its permanency.

I have now described the only constant batteries I have succeeded in producing after trying more combinations than I can remember, and will now pass to another portion of my subject.

Being aware that iron had been occasionally used instead of zinc as the positive plate of batteries, and that its electro-positiveness

varied somewhat in different solutions, I, more from curiosity than in the expectation of any useful result, substituted the blade of a knife for the zinc of one of the potash cells, thinking that perhaps it would have the effect of reducing the electromotive force to about one-half, as would have been the case had the battery been a sulphuric acid or an ammonium one. To my surprise the needle of the galvanometer was only deflected about one degree, proving that in a potash solution iron is almost as electro-negative as carbon. The utility of the discovery being at once apparent, I immediately made up a fresh cell, in which the old knife did duty for the usual carbon plate. Except that the electromotive force was slightly lower, the cell behaved in every way as well as the carbon ones. However, when it was allowed to rest for a long time, I found that a little iron was deposited on the broken carbon, due to the slight difference of potential between the iron and carbon, but on the circuit being closed this was speedily re-deposited. This double action made no perceptible difference in the working of the battery. Thinking that from their great analogy to iron in several particulars nickel and cobalt would very likely behave in a similar manner, I next made a trial of those metals. Nickel proved to be about on a par with iron, cobalt not so good. I subsequently ascertained the relative potentials of the principal metals, and carbon when immersed in hydrate of potassium, to be as follows, commencing with the most negative:—

Silver	0
Carbon	8
Platinum	8·25
Gold	8·5
Steel	9·5
Soft iron	10
Nickel	10·25
Cobalt	12
Copper	14
Arsenic	35
Tin	52
Lead	53
Zinc	60

Silver is, therefore, the most powerful negative that can be used with a potash solution. Iron behaves in a similar manner with hydrate of sodium, but is, as far as I can discover, electro-positive in all other solutions.

The disadvantages attendant on the use of carbon plates, viz., their costliness, liability to fracture, and the very great difficulty of making a good and lasting contact, which has been so much commented upon during the late discussion on galvanic batteries, are thus, by the substitution of iron, entirely done away with. A plate of carbon of the size used in No. 3 Leclanché costs at least 3*d.*, an iron plate scarcely a farthing. The carbon plate, after a time, becomes useless, owing to breakage or failure of contact. In caustic potash the iron does not rust, and will therefore prove lasting, and the binding screw can be cast into the plate so as to insure a good and permanent connection. True, that it will take twenty-one iron cells to do the work of twenty carbon ones, but if a slip of silvered iron—the cost of which would be but small—is used for the collecting plate, that objection also vanishes, and we are in a position to dispense altogether with carbon, having obtained a much more efficient substitute without additional expense.

Frequent reference has been made to the scaling and ultimate failure of the porous cells used in the Leclanché. I do not find that anything of the kind occurs in the potash battery. There are on the table some porous cells which have been in use for eighteen or twenty months. Though somewhat discoloured, there is no appearance of deterioration, and I should have no hesitation in using them again. Another advantage possessed by the potash battery is that the broken carbon contained in the porous cell, not having to be separated from any exhausted peroxide of manganese, can be used over and over again, it being only necessary to give it a good washing.

Now, as to cost. Mr. Walker states the expense of maintaining the Leclanchés in use on the South Eastern Railway to be 7*d.* per cell per annum, exclusive of wages, travelling expenses, &c. The potash battery will consume, when working on a busy line, about 8 oz. of potash in the year, the cost of which is $1\frac{3}{4}$*d.* Pyrogallic

acid, when bought in quantity, is about 1*d.* an oz. It is exceedingly light stuff; four of these bottles, as you see, being required to contain 1 oz.; and so little of it is required that one farthing's-worth will be ample for half-a-dozen cells. Nitrate of cobalt is about the same price, but more of it is wanted to secure the permanency of the battery. As the iron plate, porous cell, and broken carbon can be re-utilised, one farthing, to cover breakages, will be sufficient for those items. Mercury is not required, and, allowing 3*d.* for zinc, we have a total of a fraction of a farthing over 5*d.* But the chief saving would consist in the smaller number of cells required to do the work, especially on closed and busy short circuits. In one instance, six soda cells without pyrogallic acid replaced on a closed circuit 12 Leclanchés, with satisfactory results.

When the potash in solution has been changed to carbonate of potash by the absorption of carbonic acid the efficiency of the battery is greatly impaired. It may, however, be restored without changing the solution by dropping a small quantity of quicklime—slaked may be used, but in larger proportion—into the cell. The lime takes the carbonic acid from the potash and sinks to the bottom as carbonate of lime. Its presence does not interfere with the action of the battery.

Having enumerated the advantages of hydrate of potash, it is but fair to add that its deliquescent and caustic properties render it unpleasant stuff to handle, but as the solution can be renewed many times by means of lime its use after once setting up the battery would not be necessary except at long intervals.

Most of the preceding remarks apply equally to hydrate of sodium, but as that salt causes local action, and, owing to its creeping propensities in a dirty battery, loses its strength sooner than the potash, I have preferred to use the potash salt, though the dearer of the two. The soda, also, does not act with nitrate of cobalt.

It was my intention to have had on the table some cells representing the different combinations I have described, together with an instrument with which to test their relative strengths and constancy. But, being most unexpectedly called upon to read my paper

this evening, I am compelled to forego the pleasure of giving you a practical illustration of the results of my experiments.

Mr. HIGGINS, having called attention to two thermo-electric piles of M. Clamond of Paris, in operation on the table, said:—I have brought two of these piles here this evening, and will give a description of the manner of their construction and capabilities. These two small piles heated by gas are similar in principle to all others, no matter how they are heated. The bars are composed of an alloy of antimony and zinc, into the ends of which are cast strips of common tinned sheet-iron. The manner of making the joint between the iron and the zinc-antimony alloy is claimed as an important improvement, which assures the constancy of the pile. This alloy, when sufficiently heated, though slighly inferior in electromotive force to the alloy known as Marcus metal, gives a greater quantity of current, on account of its higher conductivity. It is also less crystalline and brittle.

The manner of heating is calculated to develop the full power obtainable. A refractory earthen tube is maintained in a red-hot state, by a number of small jets of gas mixed with air, to form Bunsen's flames issuing from it. The radiation from this tube is through the bars, and therefore, while the inner ends are being maintained at a temperature of about 400° Fah., the outer ends are at about 200°; the latter are also blackened to facilitate radiation as much as possible. Nothing else is necessary; if the difference be greater very little economy of fuel will result, because the extra heat absorbed by the pile would have to be provided by greater consumption of gas, while the destruction of the bars might be caused by the extra vibratory strain put upon them.

The bars are made as homogeneous as possible, by being cast in moulds, heated almost to the melting point of the alloy, and being allowed to cool but very slowly. The layers of bars are separated from each other by asbestos rings, and the spaces between the bars are also filled in with the same material. The tinned sheet-iron connection is in the form of a hinge, and quite sealed up inside the alloy. It is prevented from touching the side of the bar, against which it is folded, by a slip of mica.

The pressure of gas is kept constant by a small regulator, which is similar in principle to those used in street-lamps, except in having the valve supported by a bell floating in water, instead of by a membrane. The latter is preferable, because of the danger of the water evaporating unless regularly supplied.

The tube should become a light-red throughout. If heated irregularly, the current from the properly-heated portions of the pile is not assisted by the cold parts, but diminished in quantity by passing through them.

These piles were, I believe, first applied to telegraphic purposes by us. Two, of 242 and 251 bars respectively, one of which is here, were connected in series and put in place of the following batteries: two sets of 20 telegraphic Daniell's on two lines, worked with permanent currents; two sets of four Leclanché cells working the Morse instruments in connection with these two lines; and six cells bichromate battery, furnishing a constant current for a line with three Morse instruments in circuit—total = 54 cells.

They have been working since the 5th of November last. There are 108 signalling instruments altogether on these lines. Slight interruptions have taken place from bars having been broken. No stoppage would be caused if a breakage were known of and repaired at the time of its occurrence; the parts could be made to touch, until the plate had been bridged over; but if neglected the fractured surfaces become covered with a film of non-conducting oxide.

The potentials of these two piles were 12 volts. each, and internal resistance 4·50 and 6 ohms respectively. That is about 20 bars to a Daniell's cell, with a resistance of half an ohm. As there is no polarisation, these batteries can be employed up to the limit of their capabilities. One could work more than a dozen ink-writing Morse instruments, even supposing all the circuits were closed together.

The consumption of gas is from 9 to 10 cubic feet per hour. An argand flame, consuming the same quantity of gas per hour, produces very little power, and if the pile be heated by the hot air from the flame the current produced is very small indeed. It appears that nothing but radiant heat will produce a good effect.

Thermo-electric piles of this description are not well suited to replace batteries like the Leclanché and Marié Davy for telegraphic purposes, for where such elements are in use it is evident that if they do the work required of them well the actual quantity of electricity used must be small; neither could they be advantageously used where there are very few batteries. But in great telegraphic centres, where space is valuable and a number of lines can be served by the same battery, they may perhaps be useful. The danger of interruption of a number of lines resulting from the occurrence of an earth-fault on one would be very slight, on account of the low internal resistance of the pile.

A pile of 100 Daniell's potential would have a resistance equal to 25 ohms. A line with a leakage offering so low a resistance as that would necessitate in any case a special battery arrangement to suit the circumstances; therefore its removal from the universal battery would cause no inconvenience.

For submarine cables, using apparatus requiring a great quantity of electricity, they would be found convenient, and in cold places could be made to serve as stoves.

I have made some measurements of the larger piles, which serve to show that the heating by gas is more uniform on a small than a large scale. (See table, p. 185.)

There are several disadvantages attending the use of gas, such as the difficulty of obtaining it always of the same quality; its great cost; and the loss which occurs owing to a portion being carried into the air (or up the chimney) unconsumed, by the draught created by the red-hot cylinder.

At the same time this upward current, though partly necessary for the combustion of the gas, cools the inside of the pile and the outside of the heater.

These defects are entirely remedied in the piles heated by coke or charcoal. The stove, which is simply a cast-iron tube open at both ends, is placed inside a pile at a distance of about two inches from the bars at the top and bottom, but a little more at the centre, on account of the radiation thence being greater than from the ends. The annular space is closed at the bottom, to prevent the heated cylinder and the inside of the pile being cooled by the cur-

rent of air, which would be otherwise caused. A conical tube, closed at the top by a movable cover, contains several hours' supply of coke. The larger end of this tube being downwards and reaching to within a short distance of the stove, permits the coke to fall readily into the fire. The lower edge of the cover is plunged in a luting of sand to prevent the combustion of the reserve of coke by stopping the draught in that direction. The outer casing, which is about two inches distant from the supply tube, is pierced for a chimney-pipe, adjusted to give the necessary draught. A damper is provided, with piles equal to 80 Bunsen's; a large indicating galvanometer is added, with the needle arranged to arrive at zero when the maximum power the pile should give is attained. This galvanometer is permanently connected with about ten bars, disposed in different parts of the pile, and shows the state of the heating at all times.

A man accustomed to this work would find no difficulty in keeping in order piles equal to 500 Bunsen's, and the coke consumed would be about (mean size elements 3,200 per pile=80 cells) 48 lbs., cost $7\frac{1}{2}d.$ per hour. The increase and decrease of power from a change in the fire of these piles is very slow indeed. On account of the stove being well inclosed its heat can only be given off to the air through the bars of the pile. In the gas-heated piles, if the gas be extinguished for a minute the power becomes greatly diminished, because of the rapid cooling of the interior by the air.

In a discussion on telegraphic batteries it might be a little out of place to speak of the application of these piles to the deposition of metals, but, as the weight of metal deposited is to a certain extent a measure of the quantity of heat converted into electricity, it may be useful in determining the relative values of these and other generators of electricity.

Taking the coke pile of 375 bars, with an internal resistance of 4·49 ohms, and potential of 14·6 volts., and electrolytic solution of 0·04 ohms resistance (8^2 feet surface), the quantity precipitated per hour would be 56·68 grains. A potential of half a Daniell is sufficient to precipitate copper, therefore this result may be multiplied by 29·2 (potential in volts. $\times 2$) to arrive at the quantity of copper

the pile could deposit if the sectional area of the liquid were increased to diminish the resistance in proportion to the reduced potential. The quantity will be 1,713 grains per hour. If we consider the coke to be pure carbon, and the consumption necessary to precipitate 1,713 grains of copper be 14,000 grains, 8·172 grains of coke are consumed for each grain of copper precipitated. From the combining equivalents, 14,000 grains of carbon should displace about 74,600 of copper. As a quantity equivalent to only 321 grains of carbon is precipitated, it appears that the useful effect is only what is due to the combustion of 2·29 per cent. of the carbon, and the loss 97·71 per cent. But it is probable that the actual loss is much less than this, because the coke used is not pure carbon, but contains a quantity of incombustible ash, and probably a lower potential than half a Daniell would precipitate copper from a solution of the sulphate. With the electro-magnetic motor which I have here the lifting-power of the pile of 90 large bars, consuming 10 feet of gas per hour, is 40 lbs. 1 foot high per minute. The coke-burning pile could, therefore, lift about 9,600 lbs. 1 foot high per hour at a cost of less than ½*d*. This is only the result which would be obtained with motors of this form. It is far from utilising the full power of the pile.

No. bars.	Res. cold.	Hot.	Potential.	Consumption per hour.
242 small . .	3·39	4·50	12·0	9 ft. gas.
231 ,, . .	—	6·50	11·0	9 ,, ,,
262* ,, . .	3· 6	5· 1	15·0	10 ,, ,,
90 large . .		0· 9	4·0	10 ,, ,,
1,150 small . .	—	20·	32·8	50 ,, ,,
Same with heating improved		—	45·0	40 ,, ,,
400 large . .	3·04	4·35	18·5	37 ,, ,,
1,250 small,		34·0	52·0	73 ,, ,,
1,150 ,, . .		31·5	50·0	
375* large . .	3·10	4·49	14·6	2 lbs. coke.
15 German silver and iron pairs . .			1 Clamond's pair.	

* These two piles were measured while hot by Wheatstone's bridge, the upper and lower portions having been made to balance.

Deflection obtained by passing current through a wire fixed over galvanometer needle from 15 German silver and iron 5°.

,,	,,	1 Clamond	4°.

This shows that the resistance of the 15 German silver and iron pairs is less than 1 Clamond's.

Length of	large	bars 2·5	inches, mean	sectional	area 0·88
,,	small ,,	2·0	,,	,,	,, 0·26.

I think the uniformly lower potential obtained from large piles is due to the asbestos rings being made of greater width than in the small piles, and so preventing the outer ends of the bars becoming so much cooled as in the latter.

Mr. Alfred Smee, F.R.S.: I am quite unprepared to make any observations before such an assembly as this, but as you have called upon me I will state a few things which occur to me at the moment. In the first place I think sufficient allowance has hardly been made from what I have seen for what I may call the natural history of the voltaic battery itself. In the first place we may notice that all the batteries which are in use have their source and power from one and the same cause, viz. the attraction of the zinc for one element of an electrolyte—water. Now taking that as the primary condition of a battery you will perceive you can get no more power than what is obtained from the amount of zinc which is dissolved. In a perfect battery you may obtain by zinc almost the exact equivalent of power for the matter consumed, in fact to a greater extent I believe than by any mechanical force which now exists. I have tested this at the Bank of England in depositing the plates from which the bank notes are printed. I have day by day and hour by hour taken the amount of consumption of zinc to power obtained, and I can hardly find that any perceptible power is lost. If we take a piece of zinc, being a good piece of zinc, we obtain as nearly as possible the amount of deposit by chemical equivalence which ought to be deposited. I think I may challenge any other natural force to give us so exact an amount of work for the source of power which originates in the battery, viz. by the attraction of zinc for the oxygen of the water. Bearing that in view, we have got one element in the construction of all

batteries, whether for the electrotype process or for electric telegraph purposes.

Now in the use of zinc you must be very careful to see that the zinc is of good quality. The quality of the zinc for the work to be done is of primary importance, for if it contains tin or other foreign materials you will lose a very large part of the force which you ought to obtain. Taking tolerably pure commercial zinc, well amalgamated, putting it by, and then re-amalgamating it in dilute sulphuric acid before use, you may obtain the greatest amount of power possible over any other mechanical power, and it is no use saying a battery will last a long or a short time because you must have that zinc dissolved; and if you say that you have used the battery and it has not consumed much zinc we may put it down as an axiom that it has not done much work. In the second place you have to dissolve the oxide of zinc which is formed. In all ordinary batteries you must have a certain amount of water to dissolve it, and, if you say you have got a small battery which has done certain work, that is as much as to say you have had 50*s.* change for a sovereign. You have not had it. If you have to do work with a battery it is essential that the oxide of zinc should be dissolved and got into solution, and therefore the magnitude of the vessel you have is literally the test of power. To such an extent is it the test of power that at the Bank of England we use a common hydrometer by which we roughly estimate the zinc dissolved; but I have made a hydrometer by which we can tell how many grains have been dissolved to a definite quantity of solution, and I can tell on the other side of the instrument how much in thickness of copper has been deposited per square inch. To be able to do that it is absolutely necessary you should have a certain amount of fluid to dissolve your oxide of zinc.

Now all these batteries tell me on the table, without going into experiments, that it is quite impossible you, as electrical engineers, can require any large amount of work to be done in each respective cell, and I ask you, when we see that, how you are obtaining your power under economical and easy circumstances? It has been stated to-night with great propriety that hydrogen becomes an

electrical-positive and interferes with your battery. I will give you a beautiful experiment illustrating that. Take a bit of coke from your fire—a bit of cinder—dip that into dilute sulphuric acid, then apply a piece of zinc to it. You find no gas has been given off for some time. Why is that? The gas is absorbed by the coke. It is absorbed by a bit of charcoal in the same way. After a time you find that the piece of coke will begin to evolve hydrogen. Keep that piece of coke three or four days and throw it into a solution of sulphate of copper; you will find the coke covered with copper, showing that the hydrogen has been absorbed by the coke or by the charcoal, and retained in that sort of chemical combination which enables you to reduce metals from their solution with great facility. That is a principle which acts in the coating of platinum finely divided or spongy on any other metal, and I made the suggestion, so to apply it, a quarter of a century ago as applicable to the coating of other metals. You see after the coating of spongy or black platinum that the hydrogen no longer adheres to the coke, but is immediately evolved. If therefore you use a very small pole of some platinized surface which would yield that hydrogen the moment it comes in contact with it, you will obtain a battery where for the amount of fluid used and amount of zinc dissolved you obtain the best result which it is possible for any human being to obtain under like circumstances.

Now, when sulphate of zinc is formed according to Graham's beautiful laws of diffusion, that sulphate of zinc is diffused throughout the surface. The sulphate of zinc or anything else formed in that mode subsides directly to the bottom; and hence in making the large plates for the ordnance maps they have a tank so deep that the sulphate of zinc formed sinks to the bottom of the tank 5 or 6 feet high, and the upper part is always relatively in the same state with regard to the sulphuric acid it contains.

Having dealt first with the positive pole, then with the negative, and then with the quality of the acid solution, we have taken a greater part of the difficulties in the maintenance of a battery, and I can but think, if telegraph engineers would use but a small wire in place of their large cells, and if they would use strong glass

vessels which cannot break, and use simply a strip of zinc and this small platinum wire for a few cells, they would find a more commodious instrument for their purpose than any yet adopted.

I have devised many forms of instruments for various purposes and for particular objects. I will take the one devised for the working of the great clock in the Exhibition of 1851. That clock required a great deal of power. It had to work rather extensive works, and had to do a large amount of absolute battery work. In that case I put the sulphuric acid and resulting sulphate of zinc in the upper part, and surrounded the outside with silver, platinized. But even in that case that difficulty which I pointed out of perfect diffusion was so great that sulphate of zinc formed about the cell, and the battery did not do the utmost amount of work which it was capable of doing; so I fell back upon some of the primitive designs. You can have no more work done in a battery than is dependent on the amount of zinc consumed. You can have no more work than what the fluid in the size of the battery is capable of dissolving the resultant sulphate of zinc, and you can have no more power than what you obtain by the evolution of the hydrogen at the other end. If you do not work all the batteries together, one battery, if exhausted before another, becomes a decomposing cell. Therefore, if you want a battery to last a long time, you must calculate the amount of work required, you must calculate the amount of zinc to be dissolved, and you must calculate the amount of fluid which is necessary to dissolve that zinc, and then having done that you will have done, in my estimation, all that can be done in a battery, and you will obtain the greatest and longest continued power at the least possible expense and with the least possible trouble.

Dr. C. W. Siemens, F.R.S.: I have listened with great pleasure to Mr. Smee, whom we look upon as one of the fathers of the galvanic battery, and I may say that I agree with him in most of his remarks. Mr. Smee maintains very properly that the zinc consumed is the true indicator of the power which the battery gives off, and that his battery leaves nothing to be desired as regards economy; but in practical telegraphy we have other conditions to fulfil: we require

a constant battery, and for that purpose we must provide the means of getting rid of the hydrogen on the negative pole as soon as it is formed; in other words, we must get rid of polarisation, which is, as is well known, a very disturbing element in the Smee battery. Mr. Smee recommends us to platinise the surface of the electro-negative plate, and I may say, from my own experience, I have found great advantage in doing so. Some years ago I experimented with a carbon-zinc battery, and found the greatest possible advantage from platinising the carbons in it. I also tried a deposit of peroxide of lead upon the carbon, which produced similar advantages of increased electromotive force and reduced polarisation. Still for telegraph work, where we deal with currents of intensity, and with nice adjustments, constancy of action becomes a principal consideration, and it is on that account that we are led to adopt apparently more complicated arrangements, such as the Daniell or the Marié Davy battery.

I listened also with great interest to the remarks of Mr. Higgins regarding another form of battery, which is a very interesting one, and which, although it is very properly brought before us as a decided novelty, is, I may say at the same time, the cause of my first success in life. Thirty-two years ago, when I first came over to this country, I had an improvement in electro-plating, and on going to the Patent Office I was told if I used the galvanic battery my improvement would not be looked at. It then occurred to me that other sources of electricity might produce the same effect, and my thoughts fell upon the thermo-battery. I then made one, and got certain effects, which, however, would not for an instant compare with the effects we have seen to-night. Still it served my purpose in demonstrating we could obtain the desired effects without the galvanic battery, and Mr. Elkington remunerated me for my improvement. Owing to these circumstances, I naturally look with some affection upon that battery, and am delighted to see it assume at last a practical shape, and a shape in which, I think, it will do well for galvano-plastic purposes. As Mr. Higgins states, this battery will probably not take the place of the galvanic arrangement for purposes where currents are active in great resistances, but for

quantity-effect I really hope it will prove successful. It might do well, for instance, for working electro-dynamic engines, and, although it seems absurd to raise heat in order to work an electro-magnetic arrangement instead of applying the heat directly for giving the powers, there are many cases in which it might be highly inconvenient to have a caloric-engine, but where an electro-motor might be usefully employed. It is no slight recommendation that this battery may be made to serve the purpose of a stove for heating a house or room, proper arrangements being made for carrying off the products of combustion, and I am glad to see it taken up by men of ability.

Mr. W. H. Preece said: Mr. President, I think it is very desirable, as Mr. Smee suggests, that the natural history of the battery should be considered by the Society, and that generally our proceedings should contain the history of the introduction of the various appliances required for telegraphic purposes. Now I will endeavour to-night to supply one or two facts connected with that history, but before I do so I should like to make some observations on the theory of the battery, because the scientific world has been engaged in a battle as fierce as the battle of the gauges, to account for the origin of the galvanic current. The two opposing parties have been in contest on this point ever since the first introduction of galvanic electricity. Volta attributed the generation of this force to the contact of dissimilar metals. Fabroni at the same time with many physicists suggested that the source of the power was to be found in the chemical action of the liquid upon the positive metal, but Volta and his followers were cast into the shade by that great apostle of the chemical theory, Faraday, supported by the French physicists and generally by his English followers. The question has again cropped up; modern physicists, among whom may be named Sir William Thomson, have introduced a theory which tends very much to render these two views practically one. Faraday brought forward a great many experiments, one or two of which Mr Sivewright has explained and illustrated, to show that it is impossible to produce galvanic currents without chemical action. Mr. Smee commenced his

observations to-night by stating that the source of power lies in the attraction which exists between zinc and the oxygen of the liquid in which it is placed. There is no doubt the attraction or combination that occurs between the molecules of zinc and the molecules of the liquid in which it is placed is a measure of the power, but it by no means follows it is the source of the power.

Now taking first the chemical view of the question, there are several great difficulties to be explained before we can accept purely and simply the chemical theory. There is no chemical affinity between zinc and sulphate of zinc, and no chemical affinity between copper and sulphate of copper, but if you put these liquids together in the Daniell's cell we have a battery of well-known electromotive force. Again in the battery just described by Dr. Siemens—the De La Rue battery—we have three conditions which in a quiescent state indicate no chemical action whatever, but if you join opposite poles you get a current and all the effects of galvanic electricity. Again in Leclanché's batteries there is no local action implying affinity of elements, and yet you get a strong galvanic current.

Now the supporters of the contact theory say that the difference of potential, which determines the current, is not due to the chemical affinity between the negative element and the liquid, but to the contact between the dissimilar bodies which constitute the cell. If there is one fact in the whole range of physical science more determined and better known than another it is the fact that when two dissimilar bodies are brought together these bodies have a difference of potential determined between them. It matters not whether it be metals, or liquids, or gases, two dissimilar bodies, whatever their physical state, when brought together invariably have a known and determined difference of potential between them. This fact lies at the base of all electrical phenomena. It is the basis of what we know as statical electricity as well as of Voltaic electricity.

The question therefore arises: What is chemical affinity? Admitting the fact that contact between dissimilar bodies determines a difference of potential, and that a similar contact produces

chemical affinity, if we say that this chemical affinity is merely a difference of potential, all the difficulties are swept from our path, all questions of difference are set at rest, and we arrive at the conclusion that this great battle which has occupied the electrical world for more than half a century is simply a battle of words. If you take the great supporter of the chemical theory, Faraday, and if you read what he has said upon it, and simply substitute for the words "chemical affinity" the words "contact, difference of potential," you will have the explanation of the theory precisely in the same way and words as now explained by Helmholtz, Sir William Thomson, and the other supporters of the modern theory. The objection raised is this, the contact theory is opposed to the science of energy. Though it is difficult to see why the mere contact of a piece of copper and a piece of zinc should determine a difference of potential, yet the eye of the mind can enable us to see in a lump of zinc a great store of energy. We know when zinc ore has been melted and formed into metal an enormous quantity of energy remains in the zinc in a potential state, and it only remains for us to explain how it is that this energy which we know to exist can be evoked by a contact of these two metals. Of course when we have to peer into invisible regions it is difficult to explain phenomena that require a considerable amount of education to understand; but if we once conceive that all the molecules of which bodies are constituted,—if we conceive that the molecules of a piece of zinc and a piece of copper are in motion in different orbits, and their bodies when brought together produce a clash of atoms, there we have conversion of energy, there is this difference of potential between those two bodies. But there is not only difference of potential to account for in a galvanic cell; —we must not only have something to start the ball rolling in the first instance, but something to keep it rolling. The modern theorists say it is contact of dissimilar bodies which determines difference of potential and starts the ball, while it is chemical action which steps in to keep the ball rolling. We have thus a clear account of the theory of the battery, and one which coincides with all the explanations given by the rival theorists.

One immense advantage of this theory is that it settles a number of vexed questions which have long worried the minds of electricians. I would ask is there any man in this room who is satisfied in his own mind with the explanation of the effect of amalgamation given generally in text-books. Some say it is a mechanical effect, others say it is chemical, and others that it is electrical; but none agree as to the real cause of the effect of amalgamation. One fact is certain, namely, that the irregular action which takes place in batteries is due to impurities in the zinc. Let us take a piece of zinc (illustrating on the board). There is a section of a piece of zinc. Let us suppose we have a piece of tin in it. Now when you have a piece of tin in contact with a piece of zinc you have two metals in contact. There is a difference of potential between them. The one becomes *plus*, the other *minus;* and if they be immersed in liquid we have the conditions for chemical decomposition. The liquid which is dissolved attacks the zinc, and we have an element which works and which tends to destroy the plate. If instead of immersing the two elements in liquid we coat them with a very thin layer of mercury, which practically is the effect when we amalgamate the plate, we have these dissimilar elements joined by a third; we have three metals in contact, which at once tends to reduce the potential to equality; the conditions of decomposition are removed, and the zinc plate is protected from the local effects of the impurities in the zinc.

There is another *vexata questio* which still troubles us, that is, polarization in batteries. We are troubled with that greatly in the battery of the form so well known by the name of the gentleman who has addressed us to-night (Smee).

What is polarization? It is simply the result of the contact of dissimilar bodies. Contact of hydrogen with platinum or carbon determines a difference of potential, and we have the elements which establish the conditions of an opposing current.

There is another curious electrical fact we may notice, which is of an opposite kind; that is, the passivity of iron. Iron when placed in nitric acid is readily attacked. If you merely touch it with a piece of platinum this action ceases, the iron becomes passive, and

cannot be attacked by acids. Why? Because contact of these dissimilar bodies has established a difference of electrical condition between them, which prevents the so-called effect of chemical affinity. This principle was applied by Davy in his sheathing for the bottom of ships, which suffered very much from the corrosion of the sea-water. He found attaching pieces of zinc to the copper-bottom of the ship had the effect of preventing this action. Why? Because contact of these dissimilar metals determined a difference of potential; the copper became in that condition in which no chemical affinity whatever existed between it and the sea-water. We have to-night had a very pretty experiment explained to us by Mr. Smee. Take a piece of coke and keep that coke at the pole of the battery, allow it to become charged with hydrogen, take it away and put it into a solution of sulphate of copper, you find a solution of copper deposited. Why? Because it is just in the condition suitable for chemical affinity.

I would, in the next place, say a few words about the forms of batteries; and in the first place I would supplement the remarks of Dr. Siemens with regard to the beautiful experiments of Mr. De La Rue before the Royal Society by stating that that gentleman, being desirous of carrying the experiments further, applied to the Post Office authorities for the use of the cells in Telegraph Street. We have a few cells in the New Post Office. We have some 23,000 in number. But there is some difficulty in getting all these 23,000 cells at work in one series, because we are never idle at the Post Office. The circuits are always at work. However, last Sunday, Mr. De La Rue, Mr. Spottiswoode, and two or three others went with me to the Post Office, and we succeeded first in getting 1,000 cells, which crept up by steps of 500 to 5,000 cells. They remained in action for about an hour, and results were produced which I cannot anticipate, because the experiments were made by Mr. De La Rue, and will form the subject of a paper to be read before the Royal Society on some early occasion.

The following Candidates were balloted for and declared duly elected :—

As Foreign Members :—

C. M. Forssman.
P. Lutterman.
H. G. Bohr.
A. Suenson.
C. P. Nielsen.
Valdemar Thomsen.
W. G. Paulsen.
T. F. Russell.
E. A. Hansen.
F. Henningsen.
Julius Petersen.

As Members :—

Captain Marshall Hall.
W. Macgregor.
E. Riddle.
A. W. Stiffe.

As Associates : —

T. E. Dallas.
S. F. Walker.

The Meeting then adjourned.

The Thirty-sixth Ordinary General Meeting was held on Wednesday, the 28th April, 1875. Mr. LATIMER CLARK, President, in the Chair.

The PRESIDENT: We will now resume the discussion on Mr. Sivewright's paper, together with Mr. Bennett's paper on new forms of batteries. Mr. Bennett's paper was read on the third evening, and those gentlemen who spoke before are entitled to speak again. I will first call upon Mr. Preece to continue the observations which he commenced at the last meeting.

Mr. W. H. PREECE: I endeavoured on the last evening when we met to lay before you the views that are now entertained by leading physicists as to the cause of that force which is the origin of the current. I pointed out that it is not to the chemical affinity which exists between the liquid and the metal elements that we are to look for the cause of the electromotive force, but to the contact which exists between the dissimilar bodies which form the battery; chemical affinity is more the effect than the cause. It has been suggested to me that if chemical action is not present as the result of affinity, so that it cannot start the battery, how can it exist to keep up the action of the battery. My reply is simply this. Many of us after some of our meetings in this room during the recent cold weather have returned to our bachelor quarters, and have there seen in the grate a heap of coals, some wood, and some paper, carefully laid for a fire. There exist in these arrangements all the conditions for the consequence of the chemical affinity of the oxygen of the air and the coal, but we might sit till doomsday before that chemical affinity would visibly show itself without extraneous aid. In fact you cannot produce heat or light from those conditions till you set in motion the chemical affinity between the oxygen and the coals by striking a light or applying heat. In the same way in batteries, such as those where no local action takes

place, we have all the elements of chemical affinity, but nothing more, because we have not applied that starting force—that kick to the ball—which is necessary to set in action the transmutation of energy growing into a current. This is done by contact.

I have one or two remarks to make upon the history of the battery. In any appliance of science to practice, or in any mechanical improvement of scientific applications, many minds in many quarters suggest various forms, all of which cannot possibly exist. Some are more profitable than, some are superior to, the rest, and thus amongst the numerous plans which have been promulgated some have been jostled out of existence, and some still exist. Now it is quite impossible in a discussion of this kind to give an accurate history of the battery. We can only select some of the most prominent points which mark certain epochs in the history of the battery, epochs or periods which have determined certain types that, with new forms of arrangement, constitute the various forms of batteries we have in use, and are examples of the survival of the fittest. Starting with the first idea of Volta, we come to the improvement in the action of the battery effected by the introduction of amalgamation. The amalgamation of the zinc plate was a great improvement, because it determined to a considerable extent the sad waste of material which occurs in batteries of the single-fluid type which have not their zincs amalgamated. The next great step in the single-fluid battery was the adoption of the platinised silver plates of Mr. Alfred Smee, whom we had the pleasure of hearing on the last occasion we met, the date of that introduction being in the year 1836. We next come to a great epoch in the history of the battery, viz.—Daniell's great improvement, by which electrical polarisation from the effects of the deposition of hydrogen upon the negative plate was remedied. After Daniell another great improvement was that of Grove, who by the substitution of nitric acid for sulphate of copper and platinum for copper increased the strength of the battery eight times. These are the principal epochs of the improvements in batteries of the world of physics; when we come to the telegraph or more practical world the improvements are not numerous. Many here

will remember that when the telegraph was first introduced we were satisfied with Mr. Smee's form of battery filled with sand, but about the year 1852 a very great improvement, one of the greatest improvements the telegraph world ever produced in batteries, was introduced by Mr. John Fuller. There are many men whose lights have been hid under a bushel, and there are many persons in this room who probably have never heard the name of Truman in connection with gutta-percha, or of Bedson in connection with iron wire. In the same way many outside the telegraph world have not heard of John Fuller; but no man has done more to raise the battery from the position it was in in 1852, to its present position, than John Fuller. He adopted in telegraphy Daniell's form, but with this improvement, instead of using amalgamated zinc plates he placed a zinc plate in one of its own salts, in fact the zinc was placed in a solution of sulphate of zinc and the copper was placed in a solution of sulphate of copper. By this means amalgamation was avoided, and also by the peculiar form introduced we succeeded in using the same trough form of battery which was employed for the sand-batteries. The only other improvement made in the form and character of the batteries used for telegraph purposes in England and English speaking countries is that of Leclanché, and certainly the Leclanché battery for the purposes for which it is intended to be used is one of the best batteries yet brought before our notice. There are many other kinds of batteries tried, many are still under trial, but in point of fact, in an English system of telegraphy, whether applied by the Post Office or by railway companies, there are only two forms of battery generally in use, viz., that of the ordinary sulphate form of Fuller and that of Leclanché. I might say in passing there are more cells in existence of the sulphate form of battery to which I allude than of any other form in use in any other country. In fact the number of cells in use is not to be numbered by thousands, but by hundreds of thousands.

The gravity battery is a battery extensively used, and is in great favour on the continent and in America. In the year 1853 Mr. John Fuller introduced the gravity battery, and in 1854

Mr. Varley patented it in ignorance of the fact that Mr. Fuller had previously introduced a similar form of battery. This gravity battery, owing to its defects, was discarded in favour of the form we have now in use. But in 1861 M. Callaud in France introduced the same form, and this was varied by Meidinger and others in Germany, and by others in other parts of the continent. As now used it is more generally known by the name of the Callaud battery than under the name of the gravity.

It is impossible to run through the list of the various forms of batteries. There are in point of fact three types or two classes —the single-fluid and the double-fluid, but it is a difficult thing now to class batteries in these categories. One of the best modes of typifying these batteries appears to me to take, not the liquids which compose them, but the mode in which the electro-polarization effects are produced or removed. Thus we can divide batteries into those which deposit gas, those which deposit liquid, and those which deposit metals, on the negative plate. In the first, those which deposit gas, may be placed the Smee form; in the second, those which deposit liquid, the Grove form; and those in which metals are deposited are typified by the Daniell form; and these three forms of batteries may be taken as types of the various forms in use.

Now it must never be forgotten that a battery, of whatever kind it is, must be especially adapted to the work it is intended to perform. It is because some men are accustomed solely to electrotyping that they cannot understand the use of such batteries for telegraphic purposes; it is because other men are engaged all their lives in telegraphy that they cannot understand why such batteries are used for electrotyping purposes. But we must remember batteries vary in different functions. One battery varies from another in electromotive force, in internal resistance, and in constancy, and in what is important to us practical telegraphists, in cost. We want to have batteries to work long and short circuits; we want batteries to work local circuits, to work the blocks on railways, and to ring bells in our houses. We want batteries to work the vast system of fast telegraphy that is

coming into use, and we want them also for duplex telegraphy; and every one of these operations requires a different battery, varying in electromotive force, in internal resistance, and in constancy; and hence it is that people cannot comprehend why we should trouble our heads about so many different forms of batteries, from the simple type of Smee to the more complicated form of Leclanché.

Speaking of the adaptability of batteries, it may be convenient here to answer a question which has been asked in the course of this discussion, that is, what is the law that is followed in adapting batteries to the working of our telegraph circuits. My answer is, there is no rule. Every circuit requires it own power. Experience and trial are the only means by which we know the proper power to be applied to a given circuit; but in order to see whether there was any rule to follow I made some inquiries, and I found that in France they have laid down a rule—not a rigid one to be applied to every case, but a kind of general rule, to be a guide to telegraph engineers. Their rule is, that to every 100 kilometres of wire 10 cells of the Marié Davy form should be applied. I also learnt the rule adopted in India, and in India they have carried theoretical telegraphy to a high pitch of perfection. In India they have adopted this rule. There, owing to the extreme length of the circuits, they use wires of various gauges. I have selected five, viz. Nos. 1, 4, 5½, 8, and 12½. Owing to the great contrast between the two seasons in India, the dry season and the monsoons, they have to provide for the difference of insulation of the line in those two seasons. Thus for 100 miles of No. 1 wire they use in the dry season 4 cells, and in the wet 6; for No. 4 in dry weather 6 cells, in wet weather 9; for No. 5½ in dry weather 8 cells, in wet 12; for No. 8 in dry weather 12 cells, in wet 18; and for No. 12½ in dry weather 32 cells, and in monsoon weather 48.

Now in order to get some idea of what we do in England under such circumstances, Mr. Eaton, superintending engineer to our central telegraph station, carefully extracted for me the following information. The aggregate length of the Morse circuits working out of Telegraph Street is 6,689 miles, and they are worked with 1,003 cells, which give 6·5 miles per cell. Of the Wheatstone

automatic circuits the aggregate length is 9,130 miles, worked with 1,900 cells, or an average of 1 cell to 4·8 miles. Of duplex circuits the aggregate length is 9,918 miles, which require 2,440 cells, being one cell for every 4 miles, so that taking the same rule as in India we find that for every 100 miles the Morse circuit requires 15, the Wheatstone 20, and the duplex 24 cells. We make no difference, however, in the number of cells used in dry and wet weather in England, simply because we cannot say that there is any distinction to be drawn between one and the other.

Having occupied your time so long, I will remark in conclusion that we cannot say we have yet reached perfection in our batteries. There is very vast room for improvement. We had an instance brought before us the other night how experience and application can search out defects and improve batteries, and, if other members would follow the same course which Mr. Bennett has taken, the probability is we should succeed in eradicating from batteries many of the defects which now exist. For instance, with the Grove's battery, why should we be compelled to clean it every day while the Callaud battery needs attention but once a week? Why should we have sulphate batteries which want attending to monthly, while the Leclanché is satisfied with attention yearly? The ideal battery is not yet in existence. Many minds in many quarters are directing attention to the various defects in batteries, and it is in this respect that our discussions do so much good, not only in this room but throughout the globe, by the Journal which is circulated amongst our members; for, inasmuch as the contact of dissimilar bodies in a battery produces the power by which they are set in action, so the contact of many minds produces a current of thought which must result in the improvement of the battery and the furtherance of our profession.

Mr. Warren De La Rue, F.R.S.: I am very happy to respond to the President's call, but if I may be permitted I would in the first place ask a question on the very interesting remarks which have just fallen from Mr. Preece; a question which I think has a practical bearing on the subject of telegraphy. I would ask whether any experiments have been made to ascertain what quantity of de-

composed water is represented per letter or per word in telegraph messages, because, if (say) 100 cells are employed in transmitting a message a long distance, and if one cell will transmit it a short distance with the same electromotive force and a less resistance, it would be interesting to know what is the absolute consumption of zinc per word, or per letter would be better, as they are the initial components of all words. The object I have in making this inquiry is this, to ascertain if possible the actual consumption of material when there is no loss by leakage, and which necessary consumption I imagine is very inconsiderable.

Mr. W. H. Preece: No actual experiments that I recollect have been directed to that special point; but we have this fact, that in the Daniell battery, whether it is in action or not, little difference is observed in the consumption of the materials. In point of fact rather less consumption takes place when the battery is in action than when it is not in action; so that when we have such a condition as that it is almost impracticable to test the actual consumption of materials in the transmission of any word or number of words. But with regard to the Leclanché battery, though no actual quantitative experiments have been made that I am aware of, we have arrived at the fact that the consumption of materials is proportional to the work done; that is, if a given quantity of zinc is consumed in sending one message, ten times that quantity would be consumed in sending ten messages. With the Daniell battery no such rule can exist because of the local action.

Mr. Higgins said that in the battery he described in his paper, and which was still working, he did not know what actual amount of local action upon the zinc was going on; but the whole weight of sulphate of copper put in was 5 lbs., and when the battery had run out he could let them know what work that quantity of material had done.

Mr. Warren De La Rue: It would be easy to introduce into both ends of a long circuit, and also into a short circuit, a little voltameter like that on the table, and thus measure absolutely and independently of loss by leakage the chemical force expended in sending a given number of letters, and telegraph engineers would do good service to science and their profession if they would avail

themselves of the opportunities at their command of making experiments on this subject.

The battery which I have brought here this evening is composed of very small elements, and I will sketch its arrangement on this board (*illustrates*). The battery of 1,080 cells in the cabinet is of this form; the cabinet containing this number is a cube of 2 ft. 7 in. and the cell is a glass tube 6 in. long and $\frac{3}{4}$ in. in diameter, with a rod of zinc 3-16 in. diameter and 4-5 in. long, passed through a vulcanised rubber cork perforated excentrically; a flattened silver wire passes by the side of the cork to the bottom of the tube; it is covered for a certain portion of its length with gutta-percha to prevent the action of the sulphur of the cork upon the silver, and its eventual destruction, and also to prevent accidental contact with the zinc by the bending of the wire; in the bottom of the tube are placed $225\frac{1}{4}$ grains chloride of silver in powder. The upper part of the tube is filled to within 1 in. of the cork with a solution of common salt, 1,752 grains to the gallon (25 grammes to 1 litre of water), an equivalent quantity of chloride of zinc, or, still better, with a solution of sal-ammoniac (23 grammes to 1 litre). The connection between the adjoining cells is made by passing a short piece of india-rubber tube (not vulcanised) over the zinc rod of one cell and drawing the silver wire of the next cell through it, so as to press it against the zinc.

In another form of battery which produces greater chemical action the cell is also a tube of about the same length, but 1 in. to $1\frac{1}{4}$ in. in diameter, in which is placed a silver wire with a rod of chloride of silver cast upon it, and filled up to the same height with a solution of common salt, or, still better, of sal-ammoniac. To prevent accidental contact between the zinc and chloride of silver a tube open at both ends is made of vegetable parchment, cemented by shellac varnish, and big enough to inclose the chloride of silver freely; to keep this cylinder in its place, the flattened silver wire is interlaced through two holes made in the upper part of it for that purpose.

One of these batteries would, if the total quantity of chloride of silver was reduced to metallic silver, evolve 1·6 litre (1,600 cub.

cent.) of mixed gases. The battery with the chloride of silver in powder in the cabinet gives off 0·2 cub. cent. of gases per minute, with a voltameter having a resistance of 11 ohms. The other form on the table with the rods gives 3·5 cub. cent. mixed gases per minute, so that the duration of the battery can be easily estimated; for example, the powder battery would last 8,000 minutes, and the rod battery 457 minutes, if kept constantly connected up with the voltameter used, but if the poles are not in connection there is no chemical action, and the battery requires no attention. The battery now before you has been at work since the beginning of November 1874.

The total cost of the battery per cell is one shilling, including the stands to hold twenty cells; the charge costs two shillings, at the rate of 4*s.* 3*d.* per troy ounce of chloride of silver; but when the chloride of silver is reduced all that is necessary to do is to wash it with hydrochloric acid, then with distilled water, and, finally, to dissolve the cleansed silver in nitric acid, and to precipitate it again as chloride; the loss of silver is very small. This battery has two of the attributes which all batteries should possess, that is to say, there is only one fluid, and the solid electrolyte (chloride of silver) is not acted upon until contact is made between the poles, consequently it requires no attention, and therefore I think it probable that it may be found of use in telegraphy, especially when leakage is reduced to a minimum, for I believe the consumption of electrolyte absolutely necessary is very small. The waste from leakage of the lines is possibly fifty times as great as the proper consumption in an ordinary battery.

This battery, however, was not designed for telegraphic purposes, but for carrying out experiments upon the electrical discharge in vacuo. In the year 1868 Mr. Gassiott asked me for advice on this subject, and I proposed this battery to him. The matter has been lying comparatively idle until the last few months, when the first 1,080 cells were put together as preliminary to experiments with 5,000, 10,000, and probably 20,000 cells, when I have no doubt we shall get very remarkable effects. We had an opportunity, through the kindness of Mr. Preece and Mr. Eaton, of making some experiments with a Daniell's battery at the Post

Office during the few hours of leisure which occur in that active establishment; by that means I was able to confirm a result which previous experiments had indicated, viz., that the discharge in air is in exact ratio to the square of the number of cells. It was measured by a very rough instrument, but it was accurate in its indications by the method employed. With 5,000 Daniell cells the spark jumped through the one-eleventh of an inch; with 1,080 cells chloride of silver in powder it was only $\frac{1}{250}$ of an inch; so that with 20,000 cells we shall most likely have a spark $1\frac{1}{2}$ inch in length, and some marvellous results.

Illustrations of the battery were then given with lowered lights, and explanations of the various phenomena were given by Mr. De La Rue as he proceeded. When the 1,080 cells were connected with Varley's accumulators, having a capacity of 47·5 microfarads, the spark produced was, he showed, sufficient to deflagrate 8 inches of platinum wire 0·005 inch at each discharge, while only $\frac{1}{10000}$th of a grain of water decomposed gave the force necessary to produce this effect.

I will now (said Mr. De La Rue) pass on to the battery composed of a rod of zinc, with a rod of chloride of silver cast on a flattened silver wire, and, instead of the stopper being made of vulcanised rubber, it is made of paraffin. It has ten cells; seven would give the same effect. The quantity of gas evolved is about $3\frac{1}{2}$ cubic centimetres per minute. The battery, as I before said, was designed with a view to experiments to throw light upon the cause of the stratification of electrical discharges in a residual vacuum, and having no immediate connection with telegraphy; still it might be interesting to the members of the Society to see the experiments.

The experiments with 1,080 cells were exhibited in glass vacuum tubes of various dimensions, which were brilliantly illuminated and explained by Mr. De La Rue. The theory founded upon these experiments was that the stratification in some measure depended upon a series of impulses in the current, in addition to the continuous flow of the current, these impulses causing fluctuations in the quantity transmitted.*

* See Proceedings of the Royal Society, No. 160, 1875. (This paper is given *in extenso* in the Appendix.)

Mr. Arthur R. Granville : It is not my intention to take up much of the time devoted to this evening's discussion ; but I shall be glad of the opportunity of referring to the theory of the action of the galvanic cell (as regards the contact-chemical question) in the production of the electric current. In Mr. Sivewright's paper there occurs the following passage : "The latest theory which has been advanced goes a long way towards reconciling the conflicting opinions which were entertained by the supporters of the contact and chemical theories. For it allows to the former that the initial action is due to the simple contact of dissimilar bodies, and to the latter that this action can be maintained only by chemism." Professor Fleeming Jenkin, in his work on "Electricity and Magnetism," refers to the same subject in a similar train of reasoning : for he says : "Perhaps"—and note he commences with a word expressive of doubt—"Perhaps it is strictly accurate to say that the difference of potential is produced by contact, and that the current which is maintained by it is produced by chemical action."

The meaning of these paragraphs evidently is that the voltaic current is momentarily generated by the difference of potential existing at the point of contact between two dissimilar metals, and that such momentary current is then prolonged or maintained by chemism only ; just as a stone set in motion down a declivity by a kick is afterwards continued in its downward motion by the simple force of gravitation.

My object in referring to this much mooted and vexed question is to point out that this theory is not as so explained the *latest*, but that an important modification has been introduced, a modification bearing evident signs of soundness, and therefore worthy of being taken notice of in the present discussion.

A difference of potential being created at the point of contact between two dissimilar metals, one of which is immersed in a liquid, we have all the elements necessary for the production of a current, if the conditions are favourable ; and those conditions are fulfilled in an ordinary voltaic cell where both metals are immersed and the liquid closes the circuit. For, supposing the liquid not to act upon

the metals, it simply becomes a conducting medium through which the current, set up by the "contact"—difference of potential—flows in conjunction with the metals themselves. Such a current partakes somewhat of the nature of a charge at first, and afterwards resolves itself into a permanent flow, but so exceedingly feeble in strength as to elude any known method of detection. The continuation of the current must take place, unless, after the first instant when circuit is closed, the contact of two dissimilar metals no longer tends to produce a difference of potential, a fact I have never heard to be the case. Because our means of detecting a current are not delicate enough we have no right to conclude, in opposition to the known laws of electricity, that a current does not exist.

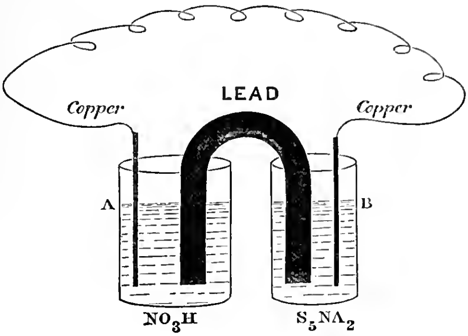
On the other hand, when the liquid is an active agent in the battery, it still serves as a conducting medium for the contact current, which it increases by its own direct chemical action on the oxidised plate. This seems to me fully supported if we can unquestionably set up a decided current in a voltaic battery without the contact of dissimilar metals. In such a case it would be reasonable to conclude that with ordinary batteries—the Daniell, the Leclanché, the Grove, or whatever other battery may be employed having two dissimilar metals for elements—that in such batteries the work performed by chemism is in no way dependent upon contact; but that contact and chemism each separately furnishes, by its own direct agency, a definite portion of the total electromotive force.

Mr. J. A. Fleming has successfully devised such a battery, described in a paper read before the Physical Society on 21 March, 1874, and printed in the Philosophical Magazine for the June following. The principle of the battery consists in arranging elements in such wise that the two terminals shall be alike. This arrangement is effected thus:

Two jars, A and B, form one cell. Into A let some nitric acid, NO_3H, be placed; into B some sodic pentasulphide, S_5NA_2. Then bend a plate of lead from one jar to the other; also place separate copper plates in each jar.

Now in nitric acid copper is negative to lead, whilst in sodic pentasulphide lead is negative to copper. Consequently, a battery

constructed as described produces an electric current flowing continuously from right to left without any contact of dissimilar



metals The conclusions arrived at by Mr. Fleming were stated in these words: "A consideration of the whole of the facts would seem to point out that the only safe conclusion is that in any series of cells of any sort the electromotive force is a complex effect, being due to the algebraical sum of all the differences of potential due to dissimilar contact, plus the algebraical sum of the differences of potential due to the chemical affinities of the metals and electrolytes, minus any opposing force due to polarisation," &c.

In effect this new theory, or rather this modification of the old theory, implies that voltaic batteries contain in themselves a double electric generating source, each separately and distinctly contributing its own absolute and definite quantity of the whole current, instead of, as hitherto taught by our text-books, the current being due to an impulse imparted to chemism by the difference of potential due to contact.

Major MALCOLM, R.E.: I think I may be allowed to make one remark with a view to correct what is evidently merely an oversight in connection with one of the points in the paper we are discussing. If I remember correctly, the white powder found in the graphite of the Leclanché has been spoken of as carbonate. As no member has drawn attention to it I would say that I have had it analysed, and it was reported to me to be a chloride, and inasmuch as the exciting liquid contains chloride of ammonium it is natural that it should be a chloride.

Mr. Preece has spoken about the contact and the chemical theory, but I think he has not left the question perfectly clear, and if I might make a suggestion it would be one by which, as it seems to me, the opposing theories may be reconciled. We allow that contact of surfaces is necessary for starting the difference of potential to which the electrical current is due. Now if we look upon the special duty of chemical action as being that of removing the surfaces which have become so to speak electrically effete, and so allowing fresh surfaces to come together, we have room to work for chemical action, while at the same time we have the contact theory uninjured.

This interesting exhibition which Mr. De La Rue has made reminds me of something I tried myself at Chatham some time ago, but as it was only incidentally part of my business I could not, I regret to say, follow it out. Of course it is understood by all that a current is but a succession of charges and discharges, but the number of fresh contacts per second which go to maintain a current is not known generally.

The calculations we made showed that it required about six thousand discharges a second to maintain a current. Of course our calculations were but rough, and many of our members who might care to take the subject up would find that many corrections would have to be made before an absolutely accurate result would be obtained; but it is to be done, and it has always appeared to me that by the use of voltaic cells of different elements that great problem—the size of molecules—may perhaps be attacked from a new direction. Finally I will inform Mr. De La Rue that I have recently been obliged to construct for my work a little spark measurer, much the same as his, but one which will measure to the $\frac{1}{20000}$th of an inch, which I shall be happy to lend him if he wants any close measurements.

Mr. Sivewright (being called upon by the President to reply upon the discussion) said: I cannot but feel flattered by the manner in which my paper has been received. If the only effect of it had been to bring before us what we have seen exhibited to-night by Mr. De La Rue, I consider that that would have exceeded any

intrinsic value which may be attached to the paper itself, and I only trust the results of those highly interesting experiments may from time to time be communicated to the Society. After all that has been stated on the subject of batteries I have very little further to remark, and at this hour of the evening it would be alike presumptuous and out of place to detain you for any length of time. Still there are one or two points which occurred to me during the progress of the discussion which I should like to mention before the question is finally dismissed.

It has been stated, oftener than once, that I have omitted to mention several forms of batteries. I am myself painfully conscious of the fact, but, if I had set to work to mention all the forms of batteries which have been tried, my paper would have been little more than a dry catalogue of galvanic combinations, and had I imposed upon myself the task of selecting these I should have had a somewhat invidious duty to perform. I therefore preferred to take those batteries which have either jostled their rivals out of existence or, not having succumbed to new-comers, are those generally employed at the present day. A wish was also expressed that suggestions for overcoming some of the defects pointed out had been offered. I was careful to avoid suggestions, for they might be multiplied without number, and, though the Society of Telegraph Engineers is no doubt the proper channel for suggestions of this kind, yet their value is slight compared with that of well-authenticated facts.

Taking up the criticism upon one or two subjects I will first refer to what M. Leclanché has written. In treating of the three defects of his battery, viz. polarisation, the formation of salts in the carbon cells, and the bursting of the porous pots, he says first of all with reference to galvanic polarisation, "As a general principle this is true, but I am in a position to state that the polarisation of a battery cell can be minimised if care be taken to increase the surface of the cell in proportion to the quantity of electricity which it is expected to supply during a given limit of time." In other words, this is to vary the size of the plates of a battery according to the work it has to do. Mr. Preece has

dwelt upon this at greater length than I shall do, although it is an important subject and closely allied to another which is equally important, viz. the proportion of the battery power to the length of the circuit. If, however, the surface of the cell is to be increased according to the amount of electricity which it is expected to supply, it would then be necessary to enlarge our battery almost indefinitely, and for every purpose required we should have to vary the size of the plates. At the same time there can be no doubt that, if the battery power were carefully apportioned to the extent required for each circuit, we should find less trouble in working them; and what Mr. Hawkins says is no doubt perfectly true, that in working circuits of high resistance the Leclanché battery answers on the whole satisfactorily.

With reference to the formation of the salt at the top of the carbon plate, M. Leclanché says, "When the carbons are thoroughly dressed with paraffin, and no moisture wets their surface by endosmosis, there can be no trace of oxidation whatever the work the battery may have to do." So long as the carbon plates can be kept carefully dry this would be true, but how are we to keep them dry? The formation of salts I believe to be inherent to the battery in its present form, and may, I think, be accounted for when we bear in mind the conditions which are present. We have first of all a cell containing a solution of chloride of ammonium. We have in this a porous vessel which contains a mixture of crushed carbon and peroxide of manganese surrounding the collecting plate of carbon: this carbon-plate is fitted with a lead cap. The porous pot is immersed in the solution, which by-and-bye makes its way through the porous partition, and must in time reach the carbon plate; by capillary action it then creeps up the carbon plate till at last it gets to the lead cap at the top; the result of course then being that we have a chloride of lead formed. The time which this takes depends upon two or three things, first upon the nature of the porous pots, and secondly upon the nature of the carbon to promote capillary action; but sooner or later it will in all probability make its appearance.

The complaints as to the bursting of the porous pots M. Leclanché

says are not well founded, and he states that with them when French earthenware is used they are not inconvenienced in this way. The remarks which several gentlemen have made on this point seem to me to give a complete answer to M. Leclanché's statement; because they all agree in this, that they have been troubled more or less by the bursting of the porous pots. French porcelain, it is stated, is to be preferred, and on this subject there appears to be unanimity of opinion that it is superior to our common earthenware. This danger in the porous pots would then to a certain extent be cured by using French ware; but that it would be eradicated altogether in this way is, I fear, too sanguine a hope to entertain. It was suggested that the evil might be encountered to some extent by plunging the pots into hot water as soon as they give indication of failing. Dipping into hot water dissolves for the time the double salts which have been formed,—and these I believe to be the main cause of the damage which is done,—but immediately the pot is put back into a lower temperature the process of crystallization begins again, and in a short time the evil becomes as bad as ever.

Mr. Rolls suggested in his letter that the failure of the porous pots might be due, to some extent, to the solvent action of the chloride of ammonium upon the earthenware. One point in favour of this view is that it is always at the bottom of the porous pot, where the solution may be supposed to be the strongest, that we first observe the failure to be taking place. Before quitting the subject of the porous pots the remark of Mr. Hawkins, that their breaking was due to the crystals of sal-ammoniac, owing to an excess of that salt being used, deserves to be noticed. Many at first sight entertain the same opinion, owing to the fact that the crystals of the double salts and those of sal-ammoniac are so like each other that without testing it is impossible to distinguish one from the other. When, however, this is done no doubt can remain as to which is the destructive agent.

With reference to the general working of the Leclanché battery several striking instances of its excellence were adduced, and they might, without doubt, be multiplied. Mr. Graves showed the

wonderful work that was done with it, but the value attached to his statement was more than counterbalanced by what was stated by him afterwards, that, when subjected to the crucial test of the duplex system, the battery failed.

With reference to the Daniell's battery there can be but one opinion—that it is excellent but expensive; no means have yet been devised which will effectually counteract the waste constantly taking place from diffusion and osmose. During the discussion there has been a tendency to regard these two as one and the same action; and in my paper the distinction between them is not so clearly laid down as it might have been. Now, although the general result of diffusion and osmose in Daniell's battery is the same, namely, unnecessary consumption of material, still it must be borne in mind that they spring from two thoroughly independent causes. Diffusion is the power which any two fluids, not chemically combining, have of mixing with each other; even when the heavier fluid is at the bottom this action goes on, and the fluids diffuse into each other according to a well-known mathematical law. Osmose is an entirely different property, and is due to the presence of the *porous partition;* in the Daniell's battery its action is evidenced by the frothing up of the cell after the battery has been at work a short time. That, however, is a subject on which very little is at present known, and it opens up a wide field of inquiry for any one who cares to go into it.

Mr. Higgins gave us a very interesting description of the thermo-electric pile, which I am sure we all duly appreciated; and I hope we shall hear further from him as to his experience with it, and whether Time, "the corrector so often where our judgments err," brings to light any substantial defects in connection with the system. I have now nothing further to say except to thank you for the kind attention which you have given to my paper, and for the general interest that has been manifested in it.

Mr. BENNETT: Having been called upon rather unexpectedly to read my paper, I should wish to make one or two supplemental remarks. I would call attention especially to the behaviour of silver in a solution of potash. Silver, in all common acids, and in

most other solutions so far as I have been able to discover, is only slightly positive, but still positive to carbon, but in a solution of potash it is as negative to carbon as carbon is to copper—in point of fact, more so—in the proportion of 8 to 6. I think a good battery on the Smee principle might be made in some such form as this.

I am now experimenting upon a silver plate in broken carbon, and it answers very well, but I am not yet able to say whether the double action which takes place will have any detrimental effect or not upon the duration of the battery. Mr. Smee stated at the last meeting a very interesting and beautiful experiment by which he proved that carbon not only absorbs but retains hydrogen. I have frequently observed that the Leclanché battery after it was once put to hard work never entirely recovered its original strength, and I have thought this was to be attributed to the formation of chloride of zinc, which made a considerable diminution in its effective strength; I now think that it is rather to be ascribed to the absorption and retention of the hydrogen by the carbon.

I have here with me to-night one of the batteries which I have already described—the pyrogallic acid cell; and I may state with regard to it that it has been on short circuit since ten minutes to two this afternoon It then gave a deflection of 30°, and it now gives a deflection of 29°. I have reason to believe, that, owing to the retention of hydrogen by carbon, no absolutely positive battery can be made in which carbon enters, and that is the reason why I have employed solid plates of iron for the carbon plates in general use.

The President: It now devolves upon me to bring to a close one of the most interesting and valuable discussions which this Society has ever had before it. I do not intend to trespass on your time by any lengthened remarks of my own, I will only briefly review the course which the discussion has taken.

Mr. Sivewright, on the occasion of the reading of his paper on the 10th of March, discussed at some length the respective merits of the contact and chemical theories. He compared the Daniell and the Leclanché batteries, pointing out their merits and defects—the

mixing of solutions in Daniell's, the want of constancy and cracking of porous pots in Leclanché's, its habit of failing suddenly, its suitability for a light current, its reduced cost and cheaper maintenance. He also called attention to the salient points in the construction of the bichromate battery, the Callaud, and Lockwood, and other forms of Daniell, as also that of Dr. Siemens and the Marié Davy.

Mr. Alfred Bennett, on the 14th of April, read a paper descriptive of a battery composed of a carbon plate packed in pieces of broken carbon as the negative element, and zinc with caustic potash as the positive. He showed that this form of battery had proved more constant than the Leclanché. He afterwards added pyrogallic acid to the negative element, or nitrate of cobalt. He showed that in a potash solution iron and nickel are found to be almost as electro-negative as carbon or platinum. He also gave the order of electro-positiveness of the different metals in a potash solution, and recommended iron, electro-plated with nickel. He estimated the cost of maintenance at 5*d.* per cell against 7*d.* for the Leclanché, and dwelt upon the advantages of the former, but he does not appear to have measured the electromotive force or the resistance.

Mr. C. V. Walker presented to us some highly useful and practical results of his personal investigations and researches into this subject. He showed that he had in operation 3,000 Leclanché cells and 9,000 cells of platinised carbon and amalgamated zinc; and that the cost of maintenance of the former was 15*d.* per annum, and that of the latter only 1*s.* $0\frac{1}{2}$*d.*, whilst it was also cheaper in first cost. He also pointed out the economy of mercury and other products and compared the duration of working and other qualities of both.

Mr. Hawkins described various improvements that have been made in the manufacture of the Leclanché battery, and treated of the local action at the lead top, the breaking up of the porous cell, and the loss of current when on short circuit.

Dr. Gladstone, in addition to some highly interesting general remarks, suggested experiments on osmose and the study of the chemistry of batteries, and described at the same time an oxygen battery, with which his name is connected.

Mr. Adams presented to us some interesting experiments and observations on the Daniell's battery.

Mr. Graves gave some valuable information on the Leclanché charged with common salt instead of sal-ammoniac, together with other interesting observations and experiences.

Mr. Higgins, of the Exchange Telegraph Company, described some batteries for special requirements, and spoke in favour of carbon batteries with bichromate of potash. He also described some improved forms of Daniell's. He exhibited two thermo-piles constructed of bars of an alloy of antimony and zinc and tinned iron; the elements are separated by asbestos; the two piles contained 150 bars, the potential being 20 bars to a Daniell's cell, with a resistance of $4\frac{1}{2}$ to 6 ohms, and a consumption of gas of 10 feet per hour. One feature in this battery was pointed out, viz., there was no polarisation. Having exhibited these coke thermo-piles, Mr. Higgins explained their chemical and mechanical results.

Mr. Alfred Smee, whose observations we listened to with great interest, in treating of the chemistry of batteries, pointed out the relation betweenzinc dissolved and work done; he dwelt upon the importance of good zinc, also upon the merits of a very small platinised pole with large vessels, and alluded generally to the law of the diffusion of liquids.

This evening we have heard some very interesting remarks from Mr. W. H. Preece, who commenced his observations at the last meeting. It is unnecessary, after what he has said, to make any remarks, for he has forestalled me in the observations I should have made. I would, however, correct him on one point, wherein I understood him to suggest that Sturgeon was the first to introduce the amalgamation of zinc. I think, myself, it was Kemp. I quite endorse all he has said about the merit that is due to Mr. Fuller for introducing the sulphate of copper battery. I remember I was at that time acting Engineer of the Electric Telegraph Company, and it was introduced to my notice by Mr. Fuller himself, and the credit of the introduction undoubtedly belongs to him, as well as the gravity battery at one time so much used.

Mr. Warren De La Rue has contributed very much to the value

of the discussion, and the experiments he has shown us to-night are of very great interest indeed, and if time permitted one might find a great deal to say upon them. The battery of chloride of silver, which he recommends, was described and its merits were appreciated by Dr. O'Shaughnessy in a little book which was published in 1853, and which shows that he quite understood the merits of the battery. Mr. De La Rue has demonstrated with how very minute a decomposition of water a powerful effect is produced in the deflagration of platinum wire. It brought to my mind the statement of Faraday, as to the very small quantity of water which would be decomposed by the force that is necessary to produce a flash of lightning, so small even as the ten-thousandth part of a grain.

For my own part, I have now scarcely anything further to add upon this subject. The contact and chemical theories are questions on which one could speak for a long time, but the question is a very obscure one, and is more fitted for a written and carefully digested paper than for verbal discussion. Mr. Fleming has written a very interesting paper on physical science, in which he combats the idea of the contact theory. I feel very much with him; I have made some experiments myself, but I found a difficulty in getting a competent experimenter, and I have not time to devote to it; still I confess I do not believe in the contact theory. I believe it will finally be proved that the action is entirely chemical, and my own feeling about a battery is, that any single element oxidisable in water forms a battery, the use of a second substance —carbon or metal—less oxidisable, being to bring on a change through which the electricity can convey itself away from the zinc. The zinc on being oxidised gives off electricity; it raises the potential of the air and lowers its own potential. As soon as no difference of potential exists all further action ceases. If you convey from the earth to the zinc the deficiency of electricity which exists in the zinc, so as to bring it up again to the same potential as the earth, then a second oxidation has taken place, and the only use of the negative element in the battery—a single-liquid or double-liquid battery—is to bring on a change by which the zinc can constantly

regain its original potential; but my own experiments on this subject have not been of such a nature as to enable me to speak with any degree of confidence upon it.

I think the thermo-battery, of which we have not heard half enough, is one of the most important inventions introduced into telegraphy for a long time, and I further think it is destined to be used not only for the purposes to which Mr. Higgins applies it but also for the purposes of telegraphy at large. I have seen batteries of moderate size, with a potential of forty or fifty cells, capable of working a number of circuits, and I think when the Post Office tries it they will find it very advantageous. I have made some experiments myself during the last two or three years, the results of which I hope before long to communicate to the Society. In those experiments I have been assisted by such men as Lawes, Lambert, Taylor, and others, and I regret that I can no longer obtain their valuable services. In conclusion, I will only remark that the gratification which this subject has afforded us has been greatly enhanced by the presence of such men as Mr. De La Rue, Mr. Smee, and Dr. Gladstone, and before we separate this evening I think it is our duty to accord a very hearty vote of thanks not only to the authors of the papers, Mr. Sivewright and Mr. Bennett, but also to those gentlemen whom I have named—especially Mr. De La Rue—for coming here and giving us the benefit of their knowledge and exhibiting before us these very interesting experiments.

The proposition was carried by acclamation, and the meeting adjourned.

APPENDIX.

(Translation.)

Paris, Sunday, 21 March, 1875.

My dear Sir,—I hasten to reply to your letter of March 19. I have just read Mr. Sivewright's paper, and I must say that, considered from a purely scientific point of view, his criticisms are not one-sided, still less unfair. The question is, substantially,

Has the best imaginable battery been invented? He replies in the negative; and I agree with him. He indicates the ideal battery thus: Required, a generator of electricity, which secures that the internal chemical action of the battery shall be in exact proportion with the external chemical action, so that this action takes place without the generator of electricity becoming weaker—in other words, without the polarisation of the battery.

Mr. Sivewright does me full justice in acknowledging that I was the first to establish the principle that it was possible to have the external and the internal chemical action in a battery proportioned to one another; but he adds, that this action cannot be brought about by our system without a weakening of the current—that is, without polarisation—when our batteries are required to produce electric currents in quantity, as, for instance, in local circuits. As a general principle this is true; but I am in a position to state that the polarisation of a battery-cell can be minimised if care be taken to increase the surface of the cell in proportion to the quantity of electricity which it is expected to supply during a given unit of time. The truth of this statement is proved by a piece of work that we have lately done in France. For nearly two years past the lighting of the gas-burners of the National Assembly has been effected by four of our cells, which have taken the place of Bunsen cells, and show no sign of polarisation. As to the complaint that the leaden tips become oxidised, as well as the screws of brass or other metal, this does not affect the chemical theory of our manganese batteries; and you know as well as I that when the carbons are thoroughly dressed with paraffin, and no moisture wets their surface by endosmosis, there can be no trace of oxidation, whatever the work the battery may have to do.

Lastly, the complaints as to the porous jars are not well founded; for in France, with our earths, we are never inconvenienced in this way.

Believe me, yours, &c.,

G. LECLANCHÉ.

John Bailey, Esq ,

The India Rubber and Gutta Percha Works.

Silvertown, Essex.

Exeter, March 22, 1875.

TELEGRAPH BATTERIES.

The Secretary,
Society of Telegraph Engineers.

SIR,—I cannot be present at the discussion upon this subject, but if you will have the kindness to bring forward the following remarks I shall be much obliged.

Mr. Sivewright, in his most valuable paper, calls attention to the common disaster in the Leclanché element of the porous pot falling to pieces, and attributes this to the mechanical action of crystals of a double salt of ammonium and zinc with chlorine, formed within the pores of the earthenware. Although this may sometimes be the case, there is evidence I think that the effect may more often be due to another cause.

I have frequently found, after breaking up a cell which had ceased to work, that the interstices of the manganese peroxide and carbon fragments were choked with crystals, and the porous diaphragm examined microscopically was seen to be in the same state, and yet evinced no sign of peeling.

On the other hand, some cells fall to pieces in a very short time, before the battery has acted long enough to produce much ammonium and zinc chloride.

I am inclined therefore to ascribe the disintegration of the pot, or division, to a solvent action of one or more of the constituents of the solution (chlorine, ammonium, or chloride of ammonium) upon some portion of the alumina compound of the cell.

Whether this view be correct or not, it will doubtless be granted that a radical cure must be effected if we can do away with the porous pot and division altogether.

This I propose to do by making the carbon plate of a pot-shape, and putting the manganese and crushed carbon inside.

The arrangement for a trough division will suggest itself.

The carbon plate, however much its pores may be choked with crystals, is never found to break; the alteration I suggest, unless some unlooked-for difficulty stepped in, should render this portion of the element almost everlasting, for when, by accumulation of

crystallised salt, the internal resistance became too high, then by removing the carbon pot and its contents, and placing them for a time in plenty of hot-water, the double salt is, as I have found by actual experiment on the ordinary cell, dissolved out, and after adding a little fresh manganese the element works as well as ever.

With regard to the destruction of the lead tops, I would suggest that this difficulty may also be got rid of, by substituting for the lead, tin or nickel, preferably the latter.

Tin could be but slightly affected by any of the contained combinations, and nickel, I venture to say, would escape all action.

Either of these metals could be deposited of the required thickness on the upper end of the carbon by electrolysis, and, as the deposit need be but small, the expense would be slight.

By piercing the carbon before plating with a hole, to take afterwards a binding-screw and nut, a very good connection with the electrotyped carbon would be made.

I am, Sir, your obedient Servant,

E. T. Rolls.

Great Western Railway Telegraph Department,
Paddington, March 24, 1875.

Dear Sir,—I regret very much I am not well enough to attend this evening, but beg to forward a few remarks and notes on the subject of Mr. Sivewright's paper, which perhaps you will be good enough to read for me.

The subject of this paper is one of very great importance to all telegraphists, and the manner the author has dealt with the descriptions of the various batteries he has named is very interesting; still I should have liked to have seen some suggestions or remedies for overcoming in a greater degree the defects he has so well pointed out to exist, especially in the Leclanché battery. There are several classes of batteries he has omitted to name, viz.: the sulphate of lead battery; the carbon battery, as used by Mr. Walker, of the South Eastern Railway; the Smee scrap battery, modified by Tyer; the Highton battery; the caustic soda and the Grenét

battery, many of which have striven to obtain a footing, but which have not equalled the modified form of Daniell's, known as the ordinary sulphate battery, or the Leclanché. I find for block-telegraph purposes no battery equal to the ordinary sulphate, though I have tried the Leclanché for blocking purposes with some success.

I believe the first four Leclanché cells that were brought to this country were brought by Mr. Vickers, of Sheffield, and through Mr. Fenton, of the Metropolitan Railway, they were given to me. I am speaking now of six or seven years ago. I put these four No. 2 cells on a block circuit between Bishop's Road and Edgware Road stations, which was continually blocked for 18 hours out of the 24, and they worked wonderfully well for about 18 months, only being refreshed with sal-ammoniac and water two or three times in that period. This so satisfied me that I ordered 100 cells from Paris, but through some arrangement then in existence they were sent me, I believe, from Belgium; these I tried, and the result was very unsatisfactory; half of them were useless, and the other half were bad and lasted only a short time. From that time until they were in the hands of the India-rubber and Gutta-percha Company, Silvertown, I did not try them, but I have since done so, and they are decidedly greatly improved, and form a valuable battery. I have tried No. 3 cells for blocking, and they have in some cases done very well indeed; in one case an 8-cell No. 3 Leclanché battery was on a disc block instrument for twelve months, at a station where it is continually pegged for eighteen hours every day; during this time it was once re-charged with sal-ammoniac; it was then taken off, because the zinc plates were worn out, new zincs were put in, and it was then put on a short single-needle circuit, and has been on for eighteen months, and is still on and working well. It has been re-charged once during this time with sal-ammoniac; it is now in good order and works very clean.

Several others, No. 3 batteries, have been tried for block circuits, where they are constantly pegged, and they have worked pretty well, but not so well as in the above case. From the experience I have had, I am not in a position to say that the Leclanché can

equal, for blocking purposes where a persistent current is kept on, the sulphate batteries; but for bell-signals or instruments not requiring a permanent current they have answered every expectation.

The following is the history of a battery on a bell-circuit. At all stations in the Paddington district an 8-cell No. 2 Leclanché battery is used on all bell-circuits. They have been on for twelve months, and nothing was done to them whatever till after they had been on more than eleven months, when some of them were recharged with sal-ammoniac, and some of them have not yet been touched. They are in first-rate working order, and look likely to work for a considerable time, being perfectly clean and free from mud. On a very busy single-needle circuit, from Gloucester to Swansea, of 102 miles, and several stations, 24 No. 3 cells were put on March 17th, 1874, and have not been touched since, and there is every appearance of their continuing to work as satisfactorily for some time to come as they have hitherto done.

The following notes will show the result of the Highton's battery, the caustic soda battery, and 12-plate sulphates on block instruments and bell-circuits:—

1. Highton's battery.—A 14-cell graphite (No. 1 cell) battery was tried on a single-needle circuit at Paddington in place of a 48-cell ordinary sulphate battery; it worked very well for two months, when it appeared to lose power all at once, and signals were reported as indistinct and not readable; it was taken off and examined, but not showing any defect it was, after a few days' rest, tried again; however after working eight days the stations again complained of weak signals. This battery has not been tried since.

2. A 6-cell caustic soda battery was tried on a disc block instrument, where it is continually pegged for eighteen hours every day; it worked very well for four months, when it failed; it was taken off and re-charged with new zincs and caustic soda; but after working three weeks it again failed, and has not been tried since.

3. Two 12-cell ordinary sulphate batteries will sometimes last on a bell-circuit for two years; they will require attention every three weeks, and are very apt to work muddy. Sulphate of copper will be required about every six weeks.

4. A 12-cell ordinary sulphate battery will work for twelve months on a disc block instrument; it will require sulphate of copper every three weeks, and it is very apt to work muddy; but in these batteries the liquid in the zinc cell should be drawn nearly off with a syringe, and filled up with water every three weeks; by this attention the zinc plates will work themselves entirely out, and little or no mud appear.

Yours faithfully,

C. E. Spagnoletti.

Geo. Preece, Esq.

Submarine Telegraph Company,
58, Threadneedle Street, London, E.C. March 24, 1875.

Dear Sir,—Being unable to attend to-night at the meeting of the Society of Telegraph Engineers, I beg to address to you a few particulars on a bichromate of potash battery, which has been under trial at the office of the Submarine Telegraph Company since the 28th of December, 1872, the practical and remarkable results of which, I think, will be interesting to the meeting.

The battery is a "one-liquid" battery: the liquid consists of $11\frac{1}{2}$ parts sulphuric acid, $5\frac{1}{2}$ bichromate of potash, 3 parts sulphate of soda, 80 parts of water. Each cell is composed of a cylindrical vessel, half the capacity of which is filled up with powdered carbon, in which a flat piece of graphite is imbedded; whilst the other half is filled up by silver-sand, in which the zinc is also imbedded.

The arrangement of the cells is peculiar, and constitutes the principal feature of the battery: the cells are placed one above the other on shelves in two, three, or more rows; on the top of each cell is placed, also on a shelf, a glass balloon vessel, at the bottom of which is fixed a porous pot. The exciting liquid is first poured into those balloons, from which it percolates slowly through the porous pot, dropping on the first row of cells; then, as the cylindrical vessel has itself a hole at the bottom, the liquid drops on the cells underneath; after having passed through these, it is finally received in vessels, to be used again. The process of percolation

takes about a fortnight to be accomplished, and the liquid, after having passed through three times, is thrown away. Now, as to the practical merits of the battery, I must say that the results of this first experiment have proved here most satisfactory. The inventor, a M. Chutaux of Paris, naturally praising his battery very highly, I resolved to put it at once to the most severe test in the practice of telegraphy. Acting according to the inventor's assertion, that 40 of his cells would do the work of at least 70 Daniell's, a battery of 40-cells Chutaux was mounted and put in action on a direct circuit between London and Paris, working a Hughes' instrument incessantly, I might say almost night and day; and since that date the battery has never failed, and at the present time continues to do the same hard work. In order to judge for yourselves, I have detached this afternoon one of the 40-cells, which I send for the inspection of the meeting. The zinc you will find very little used up for a constant action of two years and three months. The battery keeps very clean, and, probably owing to the constant motion of the liquid, I have never perceived any trace of crystallization.

As to the cost of the battery, I have not at present exact figures; but approximately it is as follows:—

Top balloon vessel . . .	10*d.*	3*s.* 8*d.* per cell, plus the cost of the liquid.
Porous pots	3*d.*	
Cylindrical vessel	8*d.*	
Graphite plate lead cap . .	10*d.*	
Zinc	10*d.*	
Sundries	3*d.*	

As to the maintenance, since November 1873 I have used about 12 lbs. of bichromate of potash, 6 or 7 lbs. of sulphate of soda, with a proportionate quantity of sulphuric acid.

I subjoin here for your information a table of the dates at which the battery was refreshed and the solution renewed.

I remain, dear Sir,

Yours truly,

A. DESPOINTES.

Secretary of the Society of Telegraph Engineers.

Chutaux Battery.

	1872.	
December	28.	Mounted.
	1873.	
January	11.	Refreshed.
,,	28.	,,
March	10.	Renewed solution.
April	4.	Refreshed
May	10.	,,
May	28.	Renewed solution.
June	17.	Refreshed
July	10.	,,
August	5.	Renewed solution.
,,	20.	Refreshed
September	8.	,,
October	4.	Half-renewed solution.
,,	25.	Refreshed
November	17.	Refreshed
,,	27.	Renewed solution.
December	13.	Refreshed
,,	30.	,,
	1874.	
January	13.	Refreshed.
February	2.	Renewed solution.
February	18.	Refreshed
March	10.	,,
April	8.	,,
May	2.	Renewed solution.
,,	19.	Refreshed
June	5.	,,
,,	16.	,,
July	12.	Renewed solution.
July	25.	Refreshed.
August	6.	,,
,,	28.	,,
September	12.	Renewed solution.
October	1.	Refreshed.
,,	26.	,,
November	15.	Renewed solution.
December	1.	Refreshed.
,,	23.	,,
	1875.	
January	8.	Renewed solution.
,,	30.	Refreshed.
February	18.	,,
March	3.	,,
,,	18.	Renewed solution.

(Translation.)

Florence, 19th April, 1875.

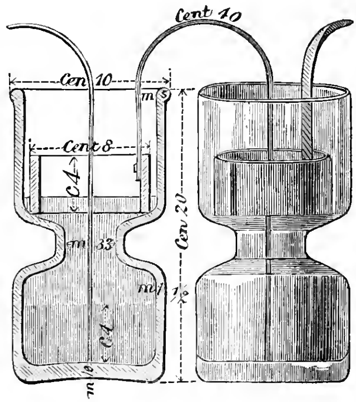
Geo. Preece, Esq.

I have received the pamphlet relating to batteries and their employment in telegraphy mentioned in your letter of 28th March, and thank you for the same.

In accordance with your request, made in the same letter, I inclose a paper upon the battery adopted by my Administration, which is a modification of that of Callaud. I may add that several years' experience has demonstrated that the battery is a very excellent one, as well for its constancy as for the economy and easiness of maintenance.

D'Amico,
Director-General.

The battery employed by the Italian Telegraph Administration is represented in the following diagram :—



Jar (*Glass*).—Height* 7·87 inches. Diameter of widest part 3·937. Internal diameter of neck 1·968. Thickness of glass, *i.e.* sides, ·177 ; rim, ·1968 ; bottom, ·3937. Weight 1·54 lbs.

Zinc.—9·055 × 1·968. Thickness ·1968. External diameter 3·1497.

Copper.—For the strip of copper shown in the figure has been

* In the diagram, as will be seen, the metrical system of measurement is employed ; in the text, however, the dimensions are given in inches.

recently substituted a copper wire ·157 in diameter, riveted at one end into a hole in the zinc plate, and terminating at the other in a short helix of four or five turns close together, and with an internal diameter of ·3937.

Preparation of the battery.—A well-saturated solution of sulphate of copper is employed for the preparation and maintenance of this battery. The jar being filled with pure water up to the neck, the zinc is inserted (previously cleaned, if it has been used before) so that the end of the copper band may touch the bottom of the adjoining jar. By means of a funnel with tube long enough, a solution of sulphate of copper is poured into the bottom of the jar so as to reach a point 1·96 or 2·36 from the lower edge of the zinc plate. The pure water first poured into the jar will then rise and cover the zinc for a space of 1·57.

Maintenance.—When the solution of sulphate in the bottom of the jar rises to within 1·18 of the zinc it is reduced by means of a fine glass syringe, so as to bring it back to the prescribed limit, and enough water is added to cover the zinc for 1·57, as already explained.

When the solution has entirely lost its colour a fresh supply of saturated solution is added by means of the funnel, a corresponding quantity of water being taken from the upper part of the jar by means of the syringe.

This operation is repeated as often as the solution is exhausted.

When the zincs are seen to be covered with a black crust which prevents their contact with the liquid they are taken out and scraped with a knife or brush, or, if necessary, renewed.

A sufficient quantity of saturated solution of sulphate of copper for two days' maintenance is always kept ready. The solution is prepared in a vessel with water at the ordinary temperature. In case of emergency hot water is used.

The sulphate to be dissolved is broken into small pieces.

(Translation.)

Vienna, 27 April, 1875.

Geo. Preece, Esq.

Sir,—In thanking you for sending me the interesting paper by Mr. Sivewright upon galvanic batteries, I have the pleasure of giving you the following information upon the batteries used in the Austrian service.

Our normal battery is the "Daniell" in two different forms. Ordinarily we employ it in the modification "Meidinger," or in the form that Mr. Sivewright calls "gravity-battery," which differs in nothing from the form also employed by the German Telegraph Administration. This form serves for both modes of working, either by the closed or the open circuit, as well as for the requirements of local circuits. It is only at the large stations, where the same battery has to supply the necessary electricity for about forty lines at the same time, that we use the Daniell element in its proper form, with a diaphragm of porous clay, and an enlarged section, so as to reduce the internal resistance.

The results of our experience obtained with the Leclanché cell were not entirely satisfactory. It exhibits too great a dependence for the force of the current upon the state of the pulverising of the carbon and of the manganese; besides the fact that the current of polarisation becomes strong when the exterior resistance diminishes, the disagreeable exhalations of ammoniacal gas, and the difficulty of maintaining the fastenings of the elements between themselves, made us renounce a more extended use of this battery.

When it becomes a question of making the battery transportable, as for its use in military telegraphy, we employ the Marié Davy element, which we have simplified by doing away with the diaphragm. It has been replaced by a mechanical obstacle which prevents the contact of the metals in the interior of the element. In spite of the cost of maintenance being more considerable for this battery, we have retained it up to the present on account of its great electromotive force and the constancy of its current.

Except the batteries mentioned above, there is only the Smee

element—zinc amalgamated and platinised silver in sulphuric acid—which we employ in some exceptional cases, when it becomes a question of reducing the internal resistance to a minimum. The form of our Smee battery permits at will the entire interruption of the galvanic action, by the separation of the metals and the liquid.

Accept the expressions, Sir, &c.,

KOLBERSTAM,

For the Minister of Commerce.

(Translation.)

Paris, 9th April, 1875.

SIR,

I have received the paper upon the Employment of Batteries in Telegraphy, inclosed in your letter of the 28th March. In thanking you for this communication I send, according to your wish, a *resumé* of the experiments that MM. Gaugain and Guillemin made in 1869 by order of the Administration. Other experiments have been recently undertaken upon the same subject; and some of them are in course of execution. The results obtained will be soon published, and I will hasten to present them to you.

Accept the expression, &c. &c.

PIERRET,

Director of the Administration.

Mr. G. Preece.

We have announced in our previous impression that the Commission had fully and carefully considered the experiments made by two of its members on the various batteries in use. It has now been decided that the reports in which Messrs. Gaugain and Guillemin have developed their interesting researches shall be forwarded to the Director-General, and that they shall be accompanied with the conclusions proposed by the President, and adopted after due discussion, as follows:—

1. Batteries of different kinds, in which zinc forms the oxidisable

element, may be employed in the telegraphic service, provided they are of suitable dimensions.

The following circumstances should be taken into consideration in selecting a battery for employment in important offices :—

The constancy of the electromotive force, the duration of the battery, the facility for maintenance, as well as the readiness with which the materials of which it is composed can be obtained, and the length and degree of insulation of the circuits to be served.

2. In each system large cells are preferable to small ones. Their electromotive force is more constant, and they last longer without attention, as they contain larger quantities of materials, whilst their electrical conductivity is greater, or, in other words, other things being equal, they furnish larger quantities of electricity than smaller cells.

3. The durability of a sulphate of copper couple does not depend solely on the useful work obtained from it, for the mixture of the solutions gives rise to a precipitation of copper on the zinc element, as well when the circuit is open as when it is closed; but owing to the constancy of this form of battery when well supplied with sulphate of copper it is desirable that its employment should be continued, especially in circumstances where great constancy is needed and but little intermission of current is likely to exist.

The Commission especially recommends that form of battery in which gravity effects the separation between the solutions, as this obviates the difficulties arising from the use of porous cells. Many forms of gravity batteries are known; those which go by the name of Callaud, with a glass cell 24 centimetres in height by 12 centimetres in diameter, have given better results than smaller ones.

The Commission considers that it would even be advantageous to use larger dimensions.

4. Sulphate of mercury (Hg_2 SO_4) batteries do not give such a constant electromotive force under varying circumstances as the last-named description, for, if the interpolar resistance does not exceed a certain limit, the electromotive force, when the circuit is

closed, diminishes as this resistance becomes less. Further, the strength of the current likewise varies from other causes, some of which have been dwelt upon in former reports of the Commission, and they likewise have the additional disadvantage of containing a highly poisonous substance.

If, however, care be taken to maintain a uniform level of the liquid in the cell, to see that there is no deficiency of the mercurial salt, to adopt suitable dimensions of the elements, and to work them on conductors offering a high resistance, good results can be obtained with them.

It is necessary that the mercurial salt used for the maintenance of these batteries shall have a uniform and invariable composition in order to avoid imperfections arising from the use of inferior materials.

Batteries of this form should preferably be of the following dimensions, these having been found more effective than smaller ones.

The exterior cell should be 0·12m. in height by 0·095m. in diameter, whilst the graphite element should be 0·14m. by 0·08m. and the zinc 0·09m. by 0·004m.

5. Batteries in which the negative carbon element is surrounded with a mixture of peroxide of manganese and sal-ammoniac may be applied to telegraphic purposes.

They are easily set up and very durable. It is necessary to observe, however, that for a certain period they diminish rapidly in electromotive force, whilst afterwards their decrease is slow but continual. When the interpolar resistance is small, the period of decrease is such that the current becomes too weak to be serviceable. Further it should be observed that simple couples, in which zinc and carbon elements are plunged in a saline solution (such as common sea salt or sal-ammoniac), have already been employed telegraphically. They do not offer a constant electromotive force when in action, and this force diminishes rapidly on circuits with small resistance, but when employed in sufficient numbers they can work telegraphic apparatus.

6. The Commission considers that it is advisable to continue the use of the various descriptions of batteries actually employed by the Administration, and to watch their working in the manner already indicated. It will be easy to judge after this series of practical trials which forms should be definitely selected.

7. In cases where the Administration is desirous of serving simultaneously a number of circuits with one battery, sulphate of copper cells of large dimensions and with a small internal resistance should preferably be chosen.

We add the following extracts from the reports of Messrs. Gaugain and Guillemin :—

Extract from M. Gaugain's Report on Batteries employed in Telegraphy.

In order to determine the value of the various batteries I have been requested to examine, I have measured the electromotive force and the resistance of each under certain of the varying conditions in which they may be placed. I propose therefore describing the methods adopted for determining the electromotive force and the resistance.

Measurement of the Electromotive Force.

I have employed for the researches about to be described the method of opposition which I have used in all previous experiments on electromotive force, and I have taken as unity the electromotive force of a thermo-electric couple, the junctions of which have been maintained, one at the temperature of zero, the other at that of 100 degrees. This is indicated by—

$$\frac{\mathrm{B}i - \mathrm{C}u}{0 - 100}$$

My measuring batteries consisted in all of 40 thermo-electric couples and three hydro-electric and one Daniell couple. Space forbidding a lengthened description of each of these cells, I

will simply add that the following shows their average relative values :—

Cells.

$$\begin{aligned} &\text{The } (Cd - Fe) = 20\,\frac{Bi - Cu}{0 - 100} \\ &\text{The } (Zn - Cd) = 62 \quad \text{do.} \\ &\text{The Daniell} \quad = 195 \quad \text{do.} \end{aligned}$$

In measuring by the method of opposition the electromotive force of a cell under given conditions a certain disposition of apparatus is necessary. As I pointed out long ago (Comptes Rendus, 24th December, 1855), the polarisation of an element falls rapidly as soon as the polarising current ceases, and therefore it is indispensable that the connection between the measuring battery and that to be measured be made as soon as the polarising current is interrupted. To this end I employ a switch, which admits of the measurement of the electromotive force of the polarised cell without interrupting the polarising current for more than the fraction of a second.

Measurement of Resistance.

Numerous methods of measuring the resistance of a battery are known, and any one of them may be employed for constant current batteries, but almost all of them fail when dealing with a battery giving a variable current, and especially when it is desirable to measure the resistance under a given condition of polarisation. In this case I measure by means of my tangent galvanometer (*multiplication conique*) the strength of the current with a known interpolar resistance, then I immediately determine the electromotive force. From this data it is easy to deduce the resistance of the battery under the given condition.

General Method of Investigation.

My experiments may be classed under two heads: the first having in view the practical determination of the variations of the electromotive force and resistance of batteries under conditions similar to

those in a telegraphic service; the second being directed towards the investigation of the causes of these variations. I confine myself on this first report to the results obtained in the former.

I have experimented with seven cells, differing one from the other either in principle or in size.

Daniell's cell	The form adopted by the Administration.
Callaud's cell	Large size.
Ditto	Small size.
Marié Davy's cell . . .	Large size.
Ditto	Small size.
Leclanché's cell	Large size.
Ditto	Small size.

I have determined the variations of electromotive force and resistance that these cells exhibit, 1st, When connected for a greater or lesser period without being in action; 2ndly, When traversed for a varying period by a constant or interrupted current of a given strength.

In comparing a set under a circulating current I have joined them all up in series, so that a current of uniform strength should traverse each.

For an interrupted current I have used Foucault's make-and-break, so adjusted as to give an average of four currents per second, the circuit being closed and opened during equal intervals of time.

I learn that it is the practice to employ about 10 cells Marié Davy for a length of 100 kilometres of line. From this I have determined the mean strength of the current employed in telegraphy, and this I have taken as the unit of current. It equals 14° of the tangent galvanometer used in these researches. This current disengages 6·35 cubic centimetres of hydrogen per hour.

Contrary to what appears in many works, the resistance of a battery is nearly independent of the strength of the current which traverses it; the electromotive force alone varies under the influence of the current, and the latter variations have primarily

engaged my attention. The following experiments were designed to show these variations :—

Researches on Electromotive Force.

I have made but one series of experiments on the electromotive force of inactive batteries. They extended from the 4th June to the 3rd November. The electromotive force was measured at least once a week, and the numerical values are recorded in one of the tables attached to the report; the general results may be stated as follows :—

Leclanché cells, large and small, lost some of their power, their mean electromotive force, which at the commencement was 285·4, being 269·8 at the conclusion of the experiments.

Marié Davy's cells of both dimensions remained unchanged, their mean value in June being 262·9, and in November 261·7.

Daniell's cells were worthless after an interval of four or five weeks, their electromotive force after this interval having become extremely weak and variable.

Callaud's small cells behaved in a like manner, only an interval of two months elapsing before they were reduced to this state; the large cells of this form were slightly weakened in power, being represented by 194·2 in June and 171·8 in November.

Five series of experiments on active batteries were made, but only two of these, as being the most complete, are dealt with in my report. They extended from May 28th to November 3rd.

The interpolar resistances were so regulated that the current always equalled the unit referred to above. The two series of experiments have been conducted under similar conditions, with the one exception, that the batteries in the one series (A) were in constant work whilst the current in the other series (B) was interrupted by means of the Foucault apparatus. The results were tabulated and attached to the report.

From these it appears that Daniell's cell behaves much in the same manner whether the circuit be closed or opened, that is, that about the same interval of time is necessary to exhaust it in either case. There is nothing surprising in this, for the destruction of

the zinc arises from the action of the sulphate of copper, and the mixture of the liquids which admits of this action takes place with about the same rapidity when the battery is quiescent as when it is active. Sulphate of copper batteries show very feeble evidences of polarisation, even under the influence of very powerful currents, and scarcely at all with those used in telegraphy.

Marié Davy's cells give varying results according to their size, the electromotive force of the large form giving a sensibly constant value for the whole duration of the experiments, that of series A being represented by 252·7 on the 1st of June and 247 on the 27th of October. On the contrary, that of the small form became nil, and even negative, in both series, in a period varying from two to seven weeks.

Sulphate of mercury batteries are strongly polarised when traversed by a powerful current, and I propose in a second report elucidating the laws which govern this; under the influence of ordinary telegraphic currents, however, very little polarity is evidenced. As already stated, Marié Davy batteries of the small form, with which I have experimented, are soon exhausted, but it should be noted that they fail almost suddenly, their electromotive force preserving their initial value whole almost up to the moment when they fail entirely.

Leclanché's large batteries, contrary to expectations, behave almost in the same manner as the smaller ones. Their electromotive force immediately falls in a notable manner, in an interval of time which does not exceed three days, and which may be much less. The mean value, which for series A was 288·7 on the 28th May, was reduced to 213·7 on the 1st June. From the latter date it gradually and slowly fell as follows :—

On the 6th August . .	199·
,, 2nd September . .	180·6
,, 3rd November . .	152·9

As may be seen, the electromotive force of the peroxide of manganese battery falls very much under the influence of the current, even when the strength of the latter does not exceed the unit used in these researches; nevertheless, the battery retains for a

sufficient length of time a sufficient power to admit of its use under certain conditions.

On Resistance.

Callaud's Batteries.—The two forms of Callaud's batteries used by the Administration offer nearly the same resistance, although their dimensions differ widely. This is easily explained by the fact that if the liquid column has a large section it likewise has an increased length.

The resistance of a Callaud's cell recently joined up is considerable, at least when, as is usually the case, the zinc element is plunged in pure water; but when the circuit is closed this water becomes charged with sulphate of zinc, and its resistance and that of the cell decreases, its minimum value equalling 5·5 units (Siemens units).

The copper deposits on the negative plate may also contribute towards diminishing the resistance by increasing the surface.

Daniell's Battery.—The resistance of Daniell's battery varies in the same manner as Callaud's. It is generally considerable immediately the battery is joined up, equalling sometimes over 60 units; but it diminishes gradually under the influence of the current, its minimum value equalling 7 to 10 units.

The decrease in the resistance of both these batteries is due to the liquid around the zinc element becoming charged with sulphate of zinc; but in the case of Daniell's cell another cause is the saturation of the porous cell with the liquids in the battery. If the porous cell be dry on its first immersion it may increase considerably the resistance of the battery, but when fully saturated it does not augment this materially.

I have made some experiments with the view of ascertaining the connection, if any, between the electrical resistance of a porous cell and the facility with which it admits of the mechanical transport of fluids; and although the experiments are incomplete they have led to a result which is interesting. It appears that two porous diaphragms which offer a very different degree of opposition to the mechanical transport of liquids may vary but slightly in

their electrical resistance when saturated. Hence the least permeable cells should be chosen for Daniell's batteries, for although I have not verified the fact it appears probable that whilst these offer the greatest resistance to the admixture of the liquids, yet when they are thoroughly saturated they do not appreciably increase the total electrical resistance of the battery.

Leclanché's Battery.—Both sizes of these cells have a similar mean average resistance, equal to 4 units when the porous cell is thoroughly saturated.

Marié Davy's Battery.—Both sizes with which I have dealt offer the same mean resistance, which differs but slightly from 6 units, when the porous cell is thoroughly saturated, and the sulphate solution contains the small proportion of pure acid determined on by the Administration. The resistance of a Marié Davy cell does not, as is generally supposed, increase when the circuit is closed. If other results have been arrived at, they were due to the battery having been used under circumstances which caused a considerable polarization, the resistance having been measured by a method applicable only to a constant battery, but it is a fact that a Marié Davy cell frequently experiences a sudden increase of resistance when it has been inactive for a considerable period, or when it is traversed by feeble currents.

Thus I have found that one of the cells of series B had acquired after five months a resistance of 65 units. As I discovered this increase only when about to terminate my experiments, I have not had time to investigate the causes, only they appear to me to be accidental, for having measured the resistance of each cell of a battery of 25 elements which had been for a long time inactive, I found that two only of the cells showed a considerable resistance, viz., 23 and 54 units respectively—whilst the 23 other cells varied between 4 and 11 units.

Conclusions.

From the above results it appears that the form of Daniell's battery actually used by the Telegraph Administration, and Callaud's small size, become unserviceable after a comparatively short period

—the former in four or five weeks, the latter in two months. They, therefore, have frequently to be recharged, and I think their use should be abandoned.

Callaud's large battery has not decreased in electromotive force more than one-tenth in five months, and it is probable that a much longer period would be necessary to exhaust it. It involves some attention, inasmuch as the sulphate of copper must be renewed as it wastes away; but the battery presents one great advantage in its power of furnishing any amount of current without becoming polarised.

Marié Davy's small battery should be abandoned, not only because it contains an insufficient supply of sulphate of mercury, but likewise because it is more powerfully polarised than the large form with the same strength of current.

Marié Davy's large cells may, I think, be employed in telegraphy in connection with apparatus like the Morse or Hughes, which necessitate full currents only, and in cases where an independent battery is used for each wire. Special investigations, however, should be made with a view of ascertaining what conditions the sulphate of mercury should fulfil in order to render its use most efficacious.

Leclanché's cell has the advantages of cleanliness, of not containing any poisonous substance, its resistance is very small, and its price will be low when the patent expires. It has the disadvantage of becoming strongly polarised when traversed by currents slightly above unit value; but this might be obviated by increasing the size of the cells.

The ammonia which is evolved when this battery is at work would become inconvenient and even dangerous for persons in its immediate vicinity if a large number of cells in constant work were employed; but if Léclanché's cells are fitted up at small offices only no fear need be entertained on this head. If necessary, steps could be taken to abolish the nuisance.

Extract from M. Guillemin's Report.

In order to serve for telegraphic purposes a battery should not lose any material portion of its energy during a period of five or six months, in addition to which its resistance should be small, on account of the defects in line insulation, which frequently reduce their resistance by a quarter, sometimes by a half, or even a larger fraction in rare cases. The battery should be such as to provide, even in unfavourable circumstances, a sufficient strength of current to work the line.

The batteries exposed to comparative trials are,—Marié Davy, sulphate of mercury; two sulphate of copper, one a Daniell with a porous partition, the other a Callaud, in which density separates the liquids; and lastly, a Leclanché, in which the depolarising agent is peroxide of manganese. Two sets of each battery have been joined up side by side, and the circuits have been closed continuously in some experiments, and discontinuously in others, either with or without interpolar resistance.

I will first give the results obtained with interrupted currents, in which the interpolar resistance was three kilometres per cell. The periods of closed and open currents were of equal duration, and four or five interruptions were made per second. The batteries worked day and night, and the electromotive force and resistance were ascertained at least twice a week. The following results were obtained:—

1. The large form of each cell is more constant than the small. It offers in general less resistance.

2. Callaud's large cell is the only constant one for many months, the electromotive force diminishing on an average by one-fortieth of its initial value during three or four months. As a counterbalance its resistance diminishes more rapidly still. It is the only battery realising these necessary conditions.

3. Daniell's battery is less constant than the last. This is due to the incrustation and rapid deterioration of the porous cell.

4. Marié Davy's battery, through two causes, rapidly becomes inefficient. When the interpolar resistance is small, its electro-

motive force falls rapidly, but its resistance decreases. When, on the contrary, the circuit offers considerable resistance, the electromotive force is constant, but the internal resistance rises considerably; so that this battery loses its power in one case by a fall of electromotive force, in another by an increase of resistance. It however recovers some of its power when the circuit is broken for some time; the diminution of electromotive force is attributable to the deposit of zinc held in solution by the mercury on the carbon plate and to the insolubility of the mercury sulphate. The increased resistance is due to the oxidation of the leaden electrode of the carbon plate, and probably likewise to the hardening of the sulphate of mercury in the porous cell.

5. Leclanché's battery is polarised a little less freely than the last-named. Its power is fairly maintained when the interpolar resistance is not too small.

The sulphate of copper cell, without a porous partition, which I propose, consists of a glass jar 24 centimetres high by 12 or 13 in diameter, its capacity being about three litres. At the bottom is placed a copper plate 1 square decimetre in surface, with a gutta-percha wire attached to it for a positive electrode. The zinc element, which should be amalgamated, is reduced to a circle 5 centimetres in radius, in the centre of the diameter of which a wire is soldered. The zinc is hung on a shelf, which is fastened to the side of the jar. The distance between the two plates is 12 centimetres. If this distance be diminished too much local action rapidly destroys the battery. The cell is closed by two glass plates, which prevent evaporation, and the consequent deposit of sulphate crystals on the sides of the glass.

The zinc should be cleaned every four months. When this is done 200 or 300 grammes of sulphate crystals are added, thus involving an annual expenditure of 1,500 grammes of sulphate of copper per cell. The solution of sulphate of zinc should at the same period be diluted by the addition of water.

A battery of this description of 24 cells has worked since the 14th November last two wires to Marseilles, two to Lyons, and one

to Bordeaux. Another battery of 50 cells has served five circuits since the 25th November, viz., those to Rouen, Lille, Havre, Tours, and Dijon. These circuits are worked by Hughes.

No confusion in the working, nor any want of power, has been discovered since their employment. The working is more regular than when each line is served by a Marié Davie battery, the cost of each element being at the outside only equal to the sulphate of mercury one. The substitution of the former for the latter, which is now almost universally employed by the Administration, would result in a considerable economy and an improved working.

Meidinger's battery, used in Germany, is likewise very constant, but its resistance is greater than the last-named, and it is three times as costly.

Other experiments lead me to the conclusion that the proposed system would be as effective with Morse as with Hughes

I will shortly give, in an extended memoir, all details of my experiments.

COMPARISON BETWEEN A MINOTTI CELL AND A No. 3 LECLANCHÉ CELL.

(*See* table on opposite page.)

Both cells were charged on the same day, and observations were taken, the one cell immediately after the other, the resistance of the galvanometer being the same in both cases. The first was simply the throw obtained from a condenser after equal charges, the second was the constant of the galvanometer through 10,000 ohms and a 20-unit shunt. The resistance of the galvanometer varied from 6054 to 6245 ohms. The highest deflections were obtained May 20 and 21, when the resistance of the galvanometer was greatest. The least deflection was when the Minotti battery was feebly charged, the resistance of the galvanometer being then lowest, although, taking the whole series, the resistance of the galvanometer does not really seem to affect the result.

G. E. Preece.

Comparison between one Minotti cell (Silvertown) and one No. 3 Leclanché cell.

Date.	Discharge from Condenser			Constant through 10,000 × $\frac{6180 \times 20}{6180+20}$ ohms.		
	Minotti.	Leclanché.	M = 100 L =	Minotti.	Leclanché.	L = 100 M =
1870, May 2	240	381	159	152	280	185
3	234	389	166	145	273	188
4	232	389	168	143	270	189
5	232	388	167	143	270	189
6	234	389	166	144	266	185
7	232	382	165	147	268	182
9	240	384	160	152	267	175
10	247	385	156	160	269	168
12	250	387	155	160	267	167
13	248	387	156	162	269	166
14	247	387	157	166	269	162
16	246	385	157	166	266	160
17	249	392	157	175	279	159
18	247	384	155	165	260	158
19	247	384	155	166	262	158
20	255	392	154	185	287	155
21	256	393	153	184	287	155
23	253	391	155	177	277	157
24	250	387	155	174	271	156
25	249	388	156	173	273	158
26	245	383	156	176	279	158
27	247	385	156	168	265	158
28	247	385	156	169	267	158
30	245	385	157	167	271	162
31	246	385	156	167	266	159
June 1	249	392	157	176	381	159
2	249	393	158	178	283	159
3	254	401	158	192	308	160
4	247	392	158	176	282	160

EXPERIMENTS TO ASCERTAIN THE CAUSE OF STRATIFICATION IN ELECTRICAL DISCHARGES IN VACUO.

By Warren De La Rue, Hugo W. Müller, and William Spottiswoode.

(*From the* Proceedings of the Royal Society, No. 160, 1875.)

Some results obtained in working with a chloride of silver battery of 1,080 cells in connection with vacuum-tubes appear to be of sufficient interest to induce us to communicate them to the Society, in anticipation of the more detailed account of an investigation which is now being prosecuted, and which it is intended to continue, shortly, with a battery of 5,000 cells, and possibly with a far greater number.

The battery used up till now consists of 1,080 cells, each being formed of a glass tube 6 inches (15·23 centim.) long and $\frac{3}{4}$ of an inch (1·9 centim.) internal diameter; each is closed with a vulcanised rubber stopper (cork), perforated eccentrically to permit the insertion of a zinc rod, carefully amalgamated, $\frac{3}{16}$ (0·48 centim.) of an inch diameter and 4·5 inches (11·43 centim.) long. The other element consists of a flattened silver wire passing by the side of the cork to the bottom of the tube, and covered, at the upper part above the chloride of silver and until it passes the stopper, with thin sheet gutta-percha for insulation, and to protect it from the action of the sulphur in the vulcanised corks; these wires are $\frac{1}{16}$ of an inch (0·16 centim.) broad and 8 inches (20·32 centim.) long. In the bottom of the tube is placed 225·25 grains (14·59 grms.) chloride of silver in powder; this constitutes the electrolyte: above the chloride of silver is poured a solution of

common salt containing 25 grammes chloride of sodium to 1 litre (1,752 grains to 1 gallon) of water, to within about 1 inch (2·54 centim.) of the cork. The connection between adjoining cells is made by passing a short piece of india-rubber tube over the zinc rod of one cell, and drawing the silver wire of the next cell through it, so as to press against the zinc.

The closing of the cells by means of a cork prevents the evaporation of water, and not only avoids this serious inconvenience but also contributes to the effectiveness of the insulation. The tubes are grouped in twenties in a sort of test-tube rack, having four short ebonite feet, and the whole placed in a cabinet 2 ft. 7 in. (78·74 centim.) high, 2 ft. 7 in. wide, and 2 ft. 7 in. deep, the top being covered with ebonite to facilitate working with the apparatus, which is thus placed on it as an insulated table.

The electromotive force of the battery, as compared with a Daniell's (gravity) battery, was found to be as 1·03 to 1,* its internal resistance 70 ohms per cell, and it evolved 0·214 cub. centim. (0·0131 cub. inches) mixed gas per minute when passed through a mixture of 1 volume of sulphuric acid and 8 volumes of water in a voltameter having a resistance of 11 ohms. The striking-distance in air of 1,080 elements between copper wire terminals, one turned to a point, the other to a flat surface, is $\frac{1}{263}$ inch (0·096 millim.) to $\frac{1}{250}$ inch (0·1 millim.) The greatest distance through which the battery-current would pass continuously *in vacuo* was 12 inches (30·48 centim.) between the terminals in a carbonic acid residual vacuum. This battery has been working since the early part of November 1874, with, practically, a constant electromotive force.

Besides 2,000 more cells like those just described, we are putting together 2,000 cells, with the chloride of silver in the form of rods, which are cast on the flattened silver wires, as in a battery described

* Compared with a Daniell's battery, in which the zinc is immersed in dilute sulphuric acid in a porous cell, its electromotive force is about 3 per cent. less than the Daniell.

by De La Rue and Müller,* but in other respects similar to the battery above described, the glass tubes being, however, somewhat larger in diameter; the rods of chloride of silver are enclosed in tubes open at the top and bottom, and formed of vegetable parchment, the object of these vegetable parchment cases being to prevent contact between the zinc and chloride of silver rods. The internal resistance of batteries so constructed is only from 2 to 3 ohms per cell, according to the distance of the zinc and chloride of silver rods, and they evolve from 3 to 4·5 cub. centim. (0·18 to 0·27 cub. inch) per minute, in a voltameter having a resistance of 11 ohms. Their action is remarkably constant.

For the experiments detailed below, vacuum tubes were generally used of about $1\frac{1}{2}$ to 2 inches (3·8 to 5 centim.) in diameter, and from 6 to 8 inches (15·24 to 20·32 centim.) long; also prolate spheroidal vessels 6 inches by 3 inches (15·24 by 7·62 centim.) The terminals are of various forms, and from 4 inches to 6 inches (10·16 to 15·24 centim.) apart, and made of aluminium, and occasionally of magnesium, and of palladium, the latter showing some curious phenomena with a hydrogen residual vacuum, which will be described in a future paper. A tube which has given the most striking results is 8 inches (20·23 centim.) long, and has a series of six aluminium rings varying in diameter from $\frac{3}{8}$ of an inch to about $1\frac{1}{4}$ of an inch (0·95 to 3·17 centim.), the thickness of the wire being about $\frac{1}{16}$ (0·16 centim.) of an inch; the rings are a little more than 1 inch (2·54 centim.) apart; and connecting wires of platinum pass through the tube from each ring, and permit of the length and other conditions of the discharge being varied.

At times the terminals of the battery were placed in connection with accumulators of different kinds—for instance, two spheres of 18 inches (45·72 centim.) in diameter, presenting each a superficies of 7·07 square feet (65·68 square decim.), and cylinders of

* Journal of the Chemical Society, 2nd series, vol. vi. p. 488; Comptes Rendus, 1868, p. 794.

paper covered with tinfoil, each having a surface of 16 square feet (148·64 square decim.); the globe and cylinders were in all cases

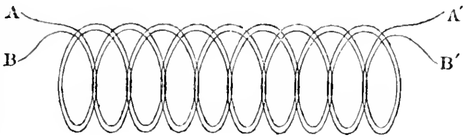


Fig. 1.

carefully insulated. Other accumulators were composed of coils of two copper wires $\frac{1}{16}$ of an inch (0·16 centim.) in diameter, covered with gutta-percha, in two folds, $\frac{1}{32}$ of an inch (0·08 centim.) thick. One coil contains two wires, A A′ and B B′ (fig. 1), coiled side by side, each being 174 yards (159 metres) long, another with two wires, each 350 yards (320 metres) long; of the latter we have two coils.

In addition to these accumulators we have several others formed of alternate plates of tinfoil and insulating material, such as paper saturated with paraffine, and also sheets of vulcanite. These are of various capacities, and contain from five to several hundred square feet. The largest has a capacity of 47·5 microfarads; when it is discharged it gives a very bright short spark, accompanied by a loud snap: the charge deflagrates 8 inches (20·32 centim.) of platinum wire, ·005 inch (0·127 millim.) in diameter, when it is caused to pass through it. Each accumulator gives different results; but for the present we shall confine ourselves to a description of the experiments made with the coil-accumulators.

When the terminals of the battery are connected with the wires of a vacuum-tube which permits of the passage of the current, the wires (especially that connected with the zinc end) become surrounded with a soft nebulous light, in which several concentric layers of different degrees of brilliancy are seen; in most cases there is either no indication of stratification, or only a feeble ill-defined tendency to stratification: the tubes selected for these experiments were those in which the stratification did not appear at all.

When the battery, already in connection with the vacuum-tube, was also joined, as in fig. 2, on to one or more coil-condensers (coupled to introduce a greater length of wire) in the following manner, then immediately well-defined stratifications appeared in the vacuum-tube.

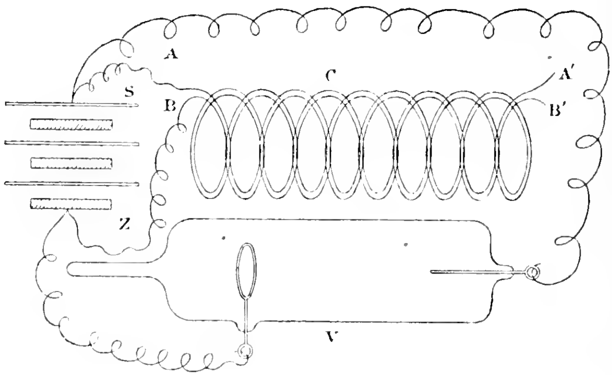


Fig. 2.

S Z represents the battery, V the vacuum-tube, C the coil-condenser; one terminal is connected with the end A of the wire A A′, and the other terminal with the end B of the second wire B B′; connexions are also led to the wires of the vacuum-tube. The ends A′ and B′ are left free; and it is clear that the coil forms a sort of Leyden jar when thus used: an interval, however short it may be, must elapse in accumulating a charge which at intervals discharges itself and causes a *greater flow* in the vacuum-tube in addition to that which passes continuously. It may be stated that the capacity of the accumulator has to be carefully adjusted to prevent any cessation of the current, to avoid, in fact, a snapping discharge at distant intervals. The periodic overflows, so to speak, which increase the current from time to time, would seem to have a tendency to cause an interference of the current-waves, and to produce nodes of greater resistance in the medium, as evinced by the stratification which becomes apparent. To the eye no pulsation in the current is apparent; and in order to convince ourselves

whether or not there was really any fluctuation in the current when the apparatus was thus coupled up with the battery, we made several experiments, and ultimately hit upon the following arrangement (fig. 3):—

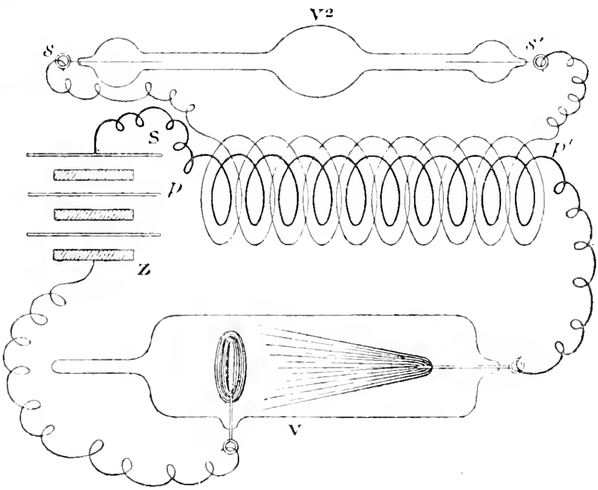


Fig. 3.

The primary wire p p′ of a small induction-coil, both with and without the iron core, was introduced into the circuit as well as the vacuum-tube V; to the secondary wire s s′ of the induction-coil was connected a second vacuum-tube, V². Under these circumstances there was no change in the appearance of the discharge in V, in consequence of the introduction of the induction-coil, the terminals being still surrounded by the soft nebulous light before spoken of: no luminosity appeared in the second vacuum-tube V² in connexion with the secondary wire of the induction-coil, except on making and breaking the connexion with the battery. At other times there was evidently no fluctuation in the continuous discharge, no periodic increase or diminution of flow, and consequently no induced current in the secondary wire s s′ of the induction-coil.

In the second experiment wires were also led from the terminals of the battery (all other things remaining as before) to the coil-accumulator, as in fig. 4; then immediately the discharge in V became stratified and the secondary vacuum-tube V² lighted up,

clearly showing that under these circumstances a fluctuation in the discharge really occurs on the appearance of stratification.

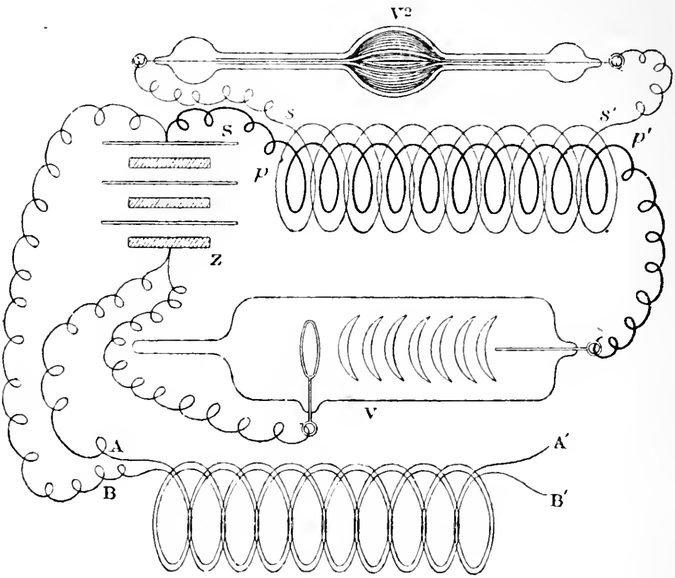


Fig. 4.

The brilliancy of the discharge in V² (the induced current passes through complicated vacuum-tubes through which the primary current cannot pass) depends greatly on the quality and quantity of the discharge in the primary vacuum-tube V. Under some circumstances the second any discharge is extremely feeble, and the illumination in V² barely visible; under others it is very brilliant.

Preparations are being made to render evident induced currents in the secondary wire of the coil too feeble to produce any illumination. Pending the further development of our investigation, we have ventured to give an account of our progress in elucidating some points in the theory of the vacuum-discharge, without any wish to ascribe to our results more weight than they deserve.

Batteries of this description may be had from Messrs. Tisley and Spiller, Brompton Road. Their cost, in large numbers, is about one shilling per cell, exclusive of the charge of chloride of

silver, which costs about two shillings per cell. The latter, either in the form of powder or of rods cast upon flattened silver wire, may be obtained from Messrs. Johnson and Matthey, Hatton Garden. When the battery is exhausted the reduced silver may be readily reconverted into chloride, with scarcely any loss.

ON A NEW THERMO-ELECTRIC PILE.

By M. C. Clamond.

The thermo-electric pile that I have termed "Thermo-Electric Generator," does not, as a whole, represent any absolutely new principle, but on the details of its construction, and the method of heating it, an essentially practical apparatus is realised, for it combines the duplicate advantages of being comparatively small, and of producing a constant and energetic current with a reasonable consumption of gas.

Before describing the technical details of my apparatus it may be well to give a retrospective glance at the whole question. The thermo-electric currents discovered by Lubeck have been searchingly investigated by various distinguished physicists, amongst others by Marcus and Becquerel. The latter has minutely studied the laws of thermo-electric currents in various substances at different temperatures, and it may be said that if his researches have not resulted in the production of a practical thermo-pile, they are not the less deserving of full recognition by all those who are occupied with thermo-electricity. The first attempt at a practical thermo-pile was made by Mr. Farmer, who exhibited two of his models at the Universal Exhibition of 1867. This apparatus, in itself of considerable note, was defective, inasmuch as it lost power rapidly, and the bars, which were very fragile, broke in cooling.

On the 31st May, 1869, M. Becquerel presented to the Institute a thermo-pile which I had constructed in conjunction with M. Mure, the couples consisting of galena and iron.

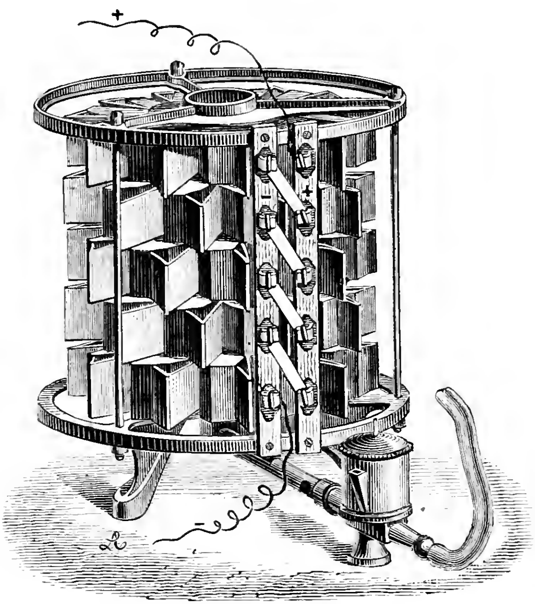
He, at the same time, proved that the weakening of the current arose not from a diminution of the electromotive force but from the gradual increase in the resistance of the apparatus. I must say, in justice to my colleague M. Mure, that if our joint efforts failed in rendering the galena pile durable, they resulted in giving to the apparatus a form and arrangement which I have since been unable to improve upon. The subsequent researches I have made have proved that the increased resistance is due to two causes:—

1. The oxidation of the points of contact of the plates under the influence of heat.

2. The lamination of the bar and its separation into plates in planes perpendicular to its length. I have obviated the first inconvenience by a particular method of connecting the couples. To effect this the metal-plate, being cut in a die, is bent round in such a manner as to form hinge-like projections. These projections being inserted in the mould in which the fusible alloy or compound is cast becomes enclosed in the latter, which likewise finds its way into the interstices of the hinges so formed. The expansion of the compound under the effects of heat by increasing the interior pressure tends to improve the contact.

The second source of failure has been found far more difficult to obviate. When a thermo-electric substance, either a metal or a sulphuret, is cast in a cubical mould, three separate planes of separation are formed parallel with the faces of the cube, so that in fact eight separate cubes are obtained. These separations are not at first visible, but after the mass has been heated repeatedly their existence is rendered clearly evident by black coats of oxide formed internally. This may be explained by the consideration of the fact that all thermo-electric substances, being deprived of their elasticity and rendered more or less brittle, are regularly crystallised on the sides of the mould. As thermo-electric substances cast in cold moulds are excessively brittle, it was thought that by annealing the bars the difficulty would be overcome. The effect of this, how-

ever, is simply to give them a superficial appearance of solidity, but it actually developes the fissures formed in the casting. I have tried thermo-piles with galena and with metallic alloys, both annealed, and as obtained from the moulds, and have invariably found that those which had been annealed lost power more rapidly than



the others. To obtain homogeneous bars the following conditions should be fulfilied. Neutralise the influence of the sides of the moulds, and prevent as far as possible all crystallization. To effect this, I have adopted the plan followed in giving solidity to, and avoiding crystallization in the manufacture of, wax candles. The mould being heated to a temperature approaching that of the point of fusion of the substance dealt with, the latter is cast at a temperature near its own point of solidification.

I have adopted for the construction of my couples the alloy of zinc and antimony employed by Marcus, together with plates of iron. I have chosen the alloy of zinc and antimony because it is a good conductor of electricity, and because its point of fusion is well

adapted to the process referred to above for casting. I must, however, in passing, allude to a fact which is opposed to the received ideas on the subject.

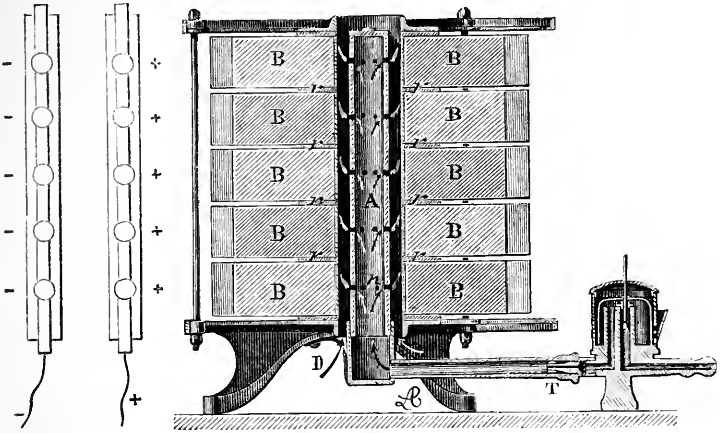
It is well known that the alloy of zinc and antimony has its highest thermo-electric power when it is composed of equal chemical equivalents of the two metals. Now experience has induced me to lower the electromotive force of my bars, so that I may gain in quantity what I lose in potential. Thus the model I have the honour to submit to the Society deposits 20 grammes of copper per hour, and the same model, constructed with an alloy which would develope a higher electromotive force, would only deposit 12 grammes per hour. This arises from the fact that the internal resistance of the bar diminishes more rapidly than its electromotive force, and thus the constant $\frac{E}{R}$ of the couple rises. Hence it follows that the employment of the most energetic bars does not necessarily supply the most energetic pile.

I employ iron preferably to copper or German silver, because the latter metals are dissolved by the alloy, and the couple is rapidly rendered useless. Iron, on the contrary, is very durable. A thermo-pile may thus be constructed which is not subject to deterioration.

I am indebted to M. Jamin for permission to set up my apparatus in his laboratory in the Sorbonne and for the facilities I have there obtained for my investigations. One of these thermo-piles was worked there for six months without undergoing any apparent change.

The following is the arrangement of the pile. The couples are joined up in a crown or circle, and connected in series. These crowns are each composed of ten couples; they are superposed and insulated from each other by asbestos. The whole forms a cylinder, whose interior is luted with asbestos, and heated by means of a clay tube perforated with holes. The mixed gas and air passing up this tube escapes through the holes, and is burnt in the annular space between it and the couples. The extremities of each crown

are connected to copper terminals, and the former can be connected in series or for quantity.



Vertical section showing the bars connected for quantity.

T Tube furnishing supply of gas.

A Clay or porcelain tube, pierced with holes N, by which the mixed gas and air escape into the annular space.

D Supply of air to support combustion.

BB Thermo-electric bars or couples.

rr Asbestos rings insulating the couples from each other.

The expenditure of gas is controlled by one of M. Giraud's regulators, which delivers a constant supply independent of variations of pressure.

Thus arranged and constructed this battery works whole months without maintenance or inspection, giving for the whole period absolutely constant currents. The model exhibited consumes 170 litres of gas, valued about 1 halfpenny (5 centimes) per hour, and deposits 20 grammes of copper in that time. This makes the cost of depositing 1 kilogramme (2·205 lbs.) 2s. 1d. (2 fr. 50 c.). A certain number of these models have been working for some months in electro-plating works.

In conclusion, I may remark that I have constructed models of various sizes. As the quantity of electricity varies with the dimensions of the couples, I have made these varying from 50 grammes to 4 kilogrammes in weight. Experience has shown that with an equal number of couples the weight of copper deposited is proportionate to the weight of the couples.

The Thirty-seventh Ordinary General Meeting was held on Wednesday, the 12th May, 1875, Mr. Latimer Clark, President, in the Chair.

The following Paper was then read:—

ON AN IMPROVED METHOD OF MEASURING BATTERY RESISTANCE.

By Frederick Hawkins, M.S.T.E.

The author of this Paper, having for some years conducted many experiments on batteries of various kinds, and applied most of the known methods for testing them, had found a great want of a better and more reliable system, combined with quickness, when a number had to be tested in a short time.

The greatest necessity was found for this change when testing the Leclanché Battery, it being found to polarise very rapidly when under the usual test.

The internal resistance of this battery could not be ascertained with anything like accuracy when tested by Fleeming Jenkin's method, where the galvanometer is shunted by a small piece of wire.

This method is the worst that can be applied for the testing of the Leclanché battery, as the whole force of the current is passed through a low resistance circuit, while obtaining the first deflection on the Thomson scale, which seriously lowers the tension before a second deflection can be obtained, in other words the battery-potential varies. The author now finds, after three years' experience, that the whole of the tests requisite in a battery can be conducted upon a sensitive Thompson galvanometer with great accuracy.

First, the electromotive force in volts (this being nothing new).

Second, the quantitative force by the application of a stout bar,

in a horizontal position at the back and about the centre of the upper coil of the galvanometer.

This bar is adjusted by a micrometer screw to regulate the position and the acting force of the quantity current.

The connecting wires to the bar are one-tenth of an inch in diameter (see thick lines on diagram). This wire being very short and stout offers little or no resistance.

When the circuit of the battery is completed in this wire, the quantity deflection is observable on the scale.

The great advantage of this is having a delicately suspended needle, perfectly independent of the internal arrangements of the galvanometer itself, to illustrate the quantity force upon an accurately graduated scale.

Another advantage is gained by the application of this bar; it is, that the polarization can be determined with great accuracy, as when the thick line circuit is complete, the fall of potential or polarization (it being short circuit) will be observed on the scale.

My method of taking internal resistance is as follows:—

The condenser being kept continually charged, and the discharge taken upon the galvanometer for the first deflection, the condenser is again charged and kept so, when, for obtaining the internal resistance, the tension on the condenser is changed by interposing a resistance in a second circuit of the battery, which acts by tapping the condenser by short contacts, until the deflection is halved.

I will now proceed to describe the key which I specially devised to meet the requirements necessary to make a series of tests on several cells with the greatest possible dispatch.

This key enables the operator to take all the necessary tests on any battery, without changing any of the connections, and being a double switch key with friction contacts is very reliable.

The large diagram represents all the apparatus with key combined.

The connections only require to be changed when interposing another battery.

To obtain force in volts, put plug in C, A being unplugged, and plug in S, pass switch E hard up to stop on N, and observe D.

Obtain deflection D with W, next deflection on B (call this d),

$$\text{then } T = \frac{d \times Y}{D}.$$

To obtain quantitative force pass switch F hard over to stop O (current passing through Q and H), and observe D. On the scale will be observed the fall which is due to polarization.

To ascertain the polarization per cent. of any battery compared with a Daniell's unit cell, after one minute's short circuit through Q and H, pass switch F hard over to stop O, observe D and time of contact, pass F quickly back to stop P, R being plugged for no resistance. This operation causes D to fall to zero, the battery still being on circuit.

At the end of one minute from first contact quickly pass switch F back hard to contact O and observe deflection on D (call this D′)

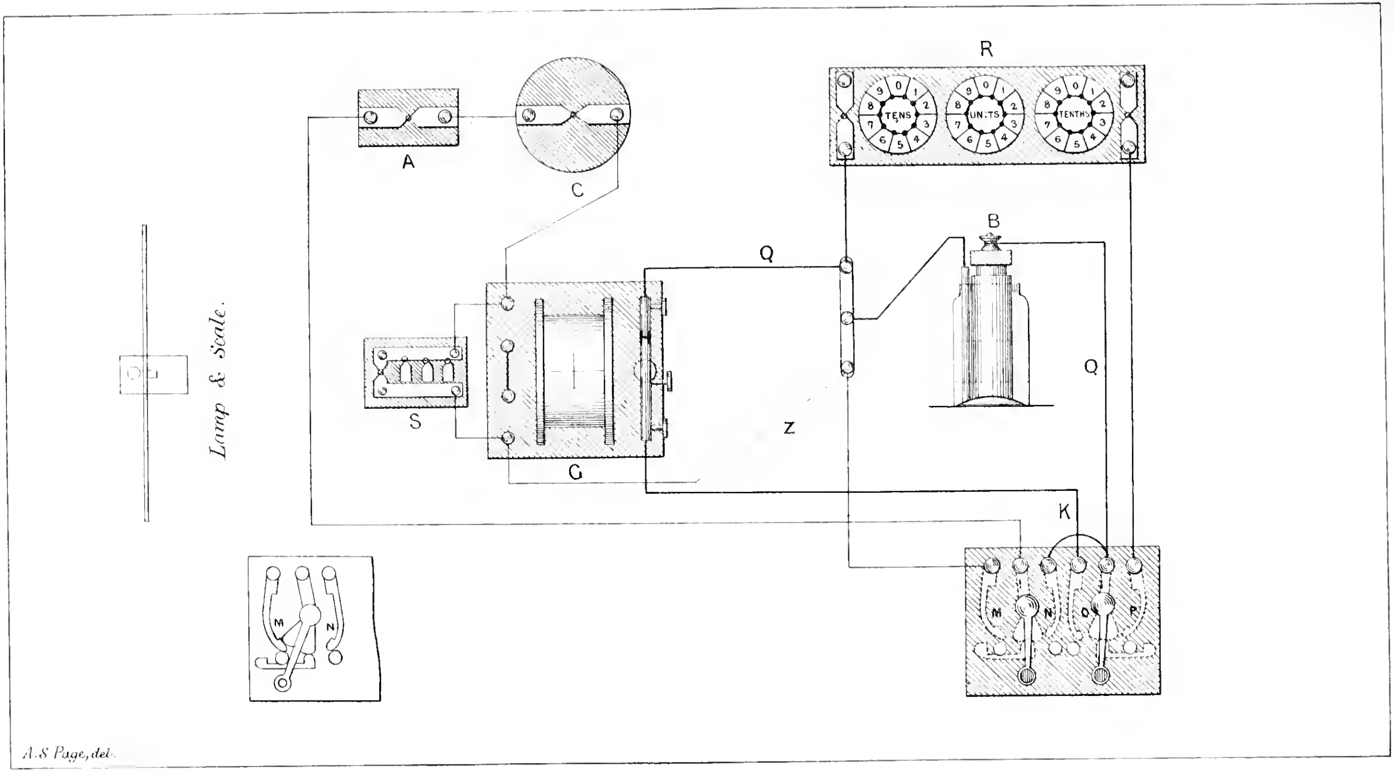
$$\text{then } U = 100 - \frac{D' \times 100}{D}.$$

To obtain internal resistance, short circuit G by plug in short circuit piece and unplug C, quickly passing switch E to M; the current passes through circuit wire Z charging C, no deflection should be observable; next remove short circuit plug and quickly pass switch E to N observing deflection D; next recharge condenser C, G being short circuited by plug in S as before, now pass switch F sharply over against stop and in contact with P, R being unplugged to what may be considered the approximate resistance of the battery.

Remove short circuit plug (no deflection should be observable), quickly pass switch F over to M and observe D, unplug resistance R to what may be considered the resistance of the battery, and pass switch F sharply over in contact with P, observing deflection D^2, being very cautious that the contact is kept on no longer than when the oscillation has attained its full extent, as during this contact Leclanché batteries are found to polarize (lever is free from contact when in the centre between O and P); should the deflection D^2 be half that of D, then the resistance unplugged in R

ARRANGEMENT FOR TESTING BATTERIES.

MR F. HAWKINS.



To face page 260.

is equal to that of B, and if the deflection is not half, the resistance in R must be varied until half of D is obtained.

Explanation of diagram.

A 10,000 ohms.
B Battery to be tested.
C Condenser $\frac{1}{3}$ microfarad.
D Deflection on scale through one megohm.
E Left lever switch.
F Right ,,
G Galvanometer resistance.
S Shunt.
H Adjustable bar for quantity and short circuit.
K Manipulating key.
M Contact for electromotive force or charge.
N ,, discharge.
O ,, quantity or short circuit.
P ,, taking internal resistance.
U Polarization per cent.
I Force in volts.
Q Stout wire quantity circuit.
Z Small wire intensity circuit.
V Internal resistance.
W Daniell unit cell.
R Resistance box.
Y Value of W in volts.

The PRESIDENT: Has any gentleman any remarks to make upon this paper? [After a pause.] I would ask just one or two questions. I understand you take the electromotive force by the swing of the condenser—by measuring the swing of the galvanometer needle from the discharge of the condenser.

Mr. HAWKINS: Not the electromotive force.

The PRESIDENT: Do you do it by comparing with another battery—you compare one current against the other?

Mr. HAWKINS: Yes, the constant current.

The PRESIDENT: When you measure your quantity by short-circuiting through the thick wire, on what form of needle do you get the deflection?

Mr. HAWKINS: On the same galvanometer.

The PRESIDENT: Is the thick wire that which conducts a certain amount of current?

Mr. HAWKINS: Yes.

The following Paper was then read:—

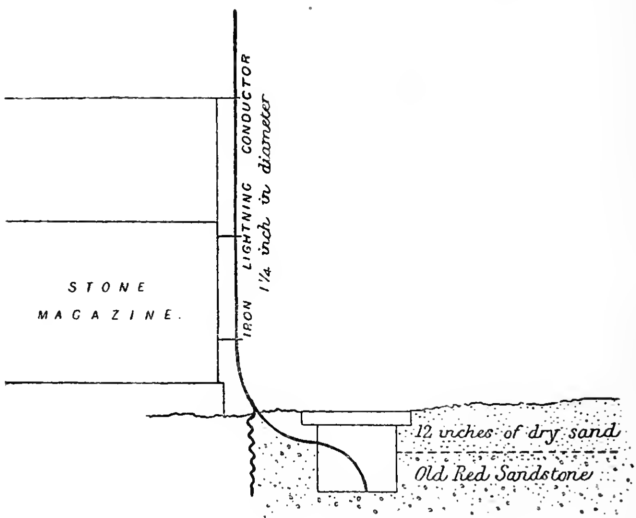
EARTH CONNECTIONS OF LIGHTNING CONDUCTORS,

By Lieut.-Colonel STOTHERD, R.E.

The danger of bad earth connections for lightning conductors is strongly exemplified in an incident the particulars of which have been given me by Mr. H. Howard, Senior Surveyor of the Department of Works, Royal Arsenal, Woolwich.

About the year 1855, a small powder magazine, close to the sea coast, at East London, Cape of Good Hope, was struck by lightning; the solid iron conductor, about $1\frac{1}{4}$ inch in diameter, was torn to pieces and the gable much injured, no damage however being done to the powder stored in the magazine. A subsequent examination showed that the iron rod forming the lightning conductor was split up and portions of it broken off, so as to render the conductor useless as a protection against lightning.

The lightning conductor, in this case, was attached to a gable of the magazine, and projected well over the top. The earth connection, if the arrangement employed can be called a connection at all, was made, as in the subjoined sketch, by carrying the lower end



of the conductor into a pit, intended to hold water permanently, but described as being frequently dry. The magazine stood on the top of a hill, about 200 feet above the sea level. The soil at this point consisted of about 12 inches of sand, overlying old red sandstone. There is but little rain at East London during the summer, sometimes even for months, and the heat is very great. Under such circumstances the ground in the neighbourhood of the magazine was necessarily very dry and a bad conductor of electricity.

There seems to be some doubt as to whether the pit into which the conductor was led was built in cement, and, even so, whether the cement was sufficiently good to make the tank watertight. The cement generally used at the Cape at that period (1855) is described as "Roman Cement;" this was sent out from England, and frequently deteriorated so much during the voyage as to arrive in a very worthless state.

Whether the tank was water-tight or not is, however, immaterial, as in either case the earth connection must have been bad. If the tank was full of water, the cement, preventing percolation through its sides and bottom, would to a great extent insulate the earth connection; if the tank was dry the conditions would be equally bad. The earth connection was therefore at fault, and there seems to be no doubt that the destruction of the lightning conductor was directly due to this fact; the charge of electricity, being unable to escape through the earth connection, was carried off at various points with disruptive effect.

There are two points connected with the case described, which demand attention.

1st. The use of tanks as a means of connecting lightning conductors with the earth.

2nd. The employment of iron for lightning conductors in substitution for copper.

As regards the first question, there seems to be no doubt that tanks are of no value as earth connections. If built water-tight, the mere fact of their preventing the escape of the water to the surrounding soil insulates the earth connection, if not entirely at least to a considerable extent. If the earth in the vicinity of the

tank is permanently dry, the fact of its being full of water does not assist in the escape of a charge of electricity, beyond the point where the conductor terminates. If the earth in the vicinity of the tank is permanently damp, it is preferable to connect the conductor directly to metallic conductors radiating from its extremity directly through the moist soil, without the intervention of a tank. For these reasons tanks, which were formerly exclusively employed in connection with lightning conductors, have been discontinued in Government buildings. Earth connections are now formed, in such cases, by means of old iron laid underground for a considerable distance and carried into damp earth.

In a letter recently received by me from Sir William Thomson, he says: "I think it would be particularly important to find whether, with a pit lined with Portland cement, and full of water, the cement would prevent earth connection. I see you assume that it would do so, but I am rather disposed to think that the moisture permeating through the Portland cement would, with sufficient area of sides and bottom, give a very good earth connection. If the ground is dry and porous outside the cement basin, then undoubtedly there would be a failure of the earth connection. In fact, it is exceedingly difficult, perhaps not possible, to get a sufficient earth connection to do away with danger from lightning on any high and dry foundation. To make a building in such a position safe, I should think it would be necessary to carry a powerful conductor down deep enough to reach a naturally moist underground stratum." It is not intended for a moment to set up an opinion in opposition to so high an authority as Sir William Thomson. His letter was written as a comment upon the solution which has just been given of the cause of the accident to the lightning conductor at East London, and it is now brought forward in the hope that some one present may be able to throw light on the subject. There are many here present who have had considerable experience in the difficulties attendant upon earth connections in working lines of electric telegraph, and these difficulties are equally to be found in the case of lightning conductors for the protection of buildings. My own impression is, that the amount

of resistance in a tank will depend directly upon the quality of the cement with which it is built; the better the cement, as a watertight envelope, the greater the resistance, an absolutely watertight tank, of course, producing the greatest impediment to the escape of an electrical discharge. The point on which information is sought is the resistance of Portland or other cement, of the best quality in a tank, which has been kept full of water sufficiently long to admit of filtration through the cement envelope, if such filtration is ever to occur.

2nd. As regards the employment of iron for lightning conductors in substitution for copper.

Though the late Sir William Snow Harris recommended the general use of copper for lightning conductors, he was not averse to the employment of iron, provided it could be secured from oxidation and decay. In a memorandum written by him for the War Department about the year 1858, he suggests the use of galvanised iron for this purpose; galvanising was then a comparatively new process, and the protection of iron lightning conductors by it was suggested by him more as an experiment than as an established system. Other electricians of eminence, and notably Sir William Thomson, have recently recommended the use of iron for lightning conductors. The accident at East London, if attributable to some other cause than a bad earth connection, might throw doubt on the suitability of this metal; but assuming that the earth connection was at fault, it does not seem to militate in any way against the use of iron, when galvanised and properly protected from atmospheric action.

Its advantages over copper are cheapness, greater resistance to mechanical injury, and higher temperature of fusion (2,786° Fahrenheit as compared with 1,994° for copper); it is, moreover, less tempting to the thief, whose operations, not being conducted on scientific principles, might produce serious consequences on a system of lightning conductors. Its disadvantages, as compared with copper, are its smaller conductivity (about one-fifth that of the latter metal), and its greater liability to oxidation and decay.

To obviate the first it would only be necessary to increase the

number and size of the conductors; the use of galvanised iron, provided the galvanising is thoroughly well executed, would go far to prevent oxidation and decay. The conductivity of zinc is to that of iron as about 8 to 5; it would therefore be an improvement in that respect. Galvanised iron rods 1 inch in diameter, or bands 2 inches wide and $\frac{3}{8}$ inch thick, would be sufficient to convey the heaviest charge of electricity likely to occur during any storm in this country to earth without danger. The dimensions of copper conductors at present recommended for Government buildings are —rods $\frac{1}{2}$ inch in diameter, tubes $\frac{5}{8}$ inch in diameter, and $\frac{1}{8}$ inch thick, and bands $1\frac{1}{2}$ inches wide and $\frac{1}{8}$ inch thick.

Mr. W. H. Preece said: Mr. President, I think there are many indications that we may anticipate severe electrical storms during the ensuing year, and we are indebted to Col. Stotherd for giving us a paper as a peg upon which to hang a discussion upon that subject. We have passed through a winter of extreme severity, and we hear from the antipodes, and from the Cape especially, that thunder storms have been unusually numerous and exceptionally severe, and it really behoves us as Electrical Engineers to call the attention of the public at large to the shameful and dreadful neglect of the simple and inexpensive means of protection which are at the disposal of all.

In the remarks we have heard this evening there are two or three points suggested which are calculated to improve our knowledge of the effects of atmospheric electricity, and of the means at our disposal to protect ourselves from them. A most interesting and admirable account appeared in a periodical which we all read, called *Nature*, written by Mr. Pidgeon, who resided near Torquay: and the subject has also been brought before the Society of Arts in an elaborate, exhaustive, and extremely able paper by Dr. Mann, the President of the Meteorological Society. I am happy to inform you that Mr. Pidgeon and Dr. Mann are present, and will both, I have no doubt, give us their experience and the benefit of their advice; but Mr. Pidgeon's communication to "Nature" is one which is so valuable that in the first place I will read it out—not so much for your own edification to-night but in

order that it may be printed in our Proceedings as a model of what scientific observations ought to be. Anything more precise, more complete, and anything more absent from theory, I have seldom read; for almost all scientific communications are ruined by vile attempts to explain phenomena which really are only explicable upon well-considered and well-known theoretical bases. We want, in this Society especially, facts; many amongst us have theoretical knowledge sufficient to mould those facts into some form that will place eventually the theory of atmospheric electricity on a footing it is not now—that is, on a footing of truth.

Mr. Pidgeon, writing to *Nature*, says:—

"The following is offered you for publication in the hope that the facts were observed accurately enough to be of value, and in the belief that reliable accounts of similar experiences are rare."

In order to make the matter clear to you I placed myself in communication with Mr. Pidgeon, and through his kindness I received a sketch of the premises and of the results of the phenomena, which I have drawn in the diagram behind the President. That is a sketch (page 268) of the house and grounds in which the accident took place

The explanation marked on the sketch will enable you to follow the diagram, and the diagram will assist in following the communication.

"The house in which with my family and I have spent the winter stands in the centre of Torbay and close to the sea. In the garden, which gives access to the shore, is a flagstaff (once belonging to the Coast Guard) 50 feet high, with a metal vane at the top, and having the mast steadied at about 25 feet from the ground in the usual way with iron-wire guys. About a foot above ground each wire rope terminates in a $\frac{1}{2}$-inch chain, which is anchored a few feet in the soil. These chains are much corroded, their original diameter being reduced here and there to $\frac{1}{8}$-inch.

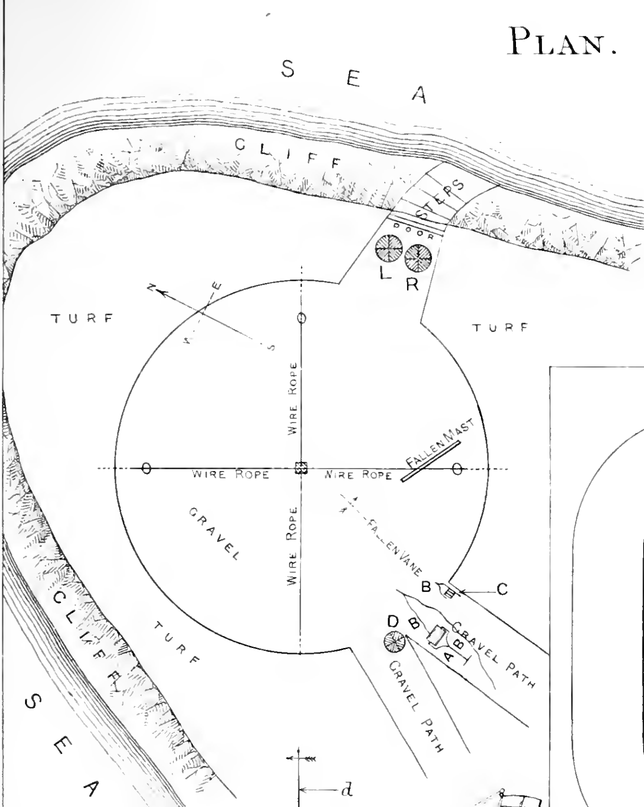
"February 25th was a rainy day during the forenoon, with heavy wind from the south-east, but in the afternoon the sky cleared. There had been no sign of thunder all day. At 5 P.M. my wife, my son, and myself were standing under the flagstaff and

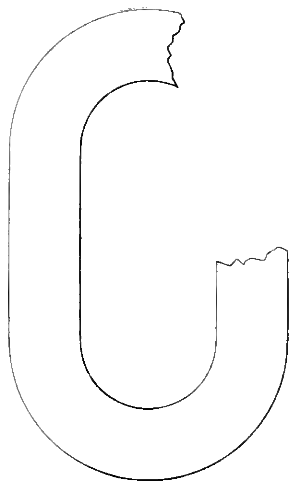
within 10 feet of a mooring chain, watching the bay, when the vane was suddenly struck by lightning, which broke the mast short off in two places, tearing and splitting the wood between the vane and the iron guy ropes. Through these the discharge then passed to the ground, but three out of the four mooring chains were broken. Not only one, but many links in each of these chains were snapped, both above and below ground, and several of the links were broken in two places at once. The fractures were crystalline and showed no signs of heat. On the garden path, and within a yard of myself, stood an iron roller, towards which the discharge ploughed two shallow furrows in the gravel; one of these is 8 feet long and terminates in a splash of gravel upon the roller.

"The broken mast and vane fell to the ground close to us. The former was blackened from end to end around half its circumference, and the edges of the discolouration form ragged splashes. The brass tube forming the vane was ripped open, and all solder about the vane melted. Below the point where the wire-ropes were attached to it the mast was uninjured. Shivered fragments of the staff were found on the ground as far as 150 feet to windward. Heavy hail followed the flash, the wind falling instantly to a dead calm; a second but distant flash was seen twenty minutes later, after which there was no more lightning. The discharge startled the whole village of Paignton; the coastguard officer compares the explosion to that of a 300-pounder gun; and at Torquay, $3\frac{1}{2}$ miles distant, a scientific friend speaks of both flash and crash as most terrific.

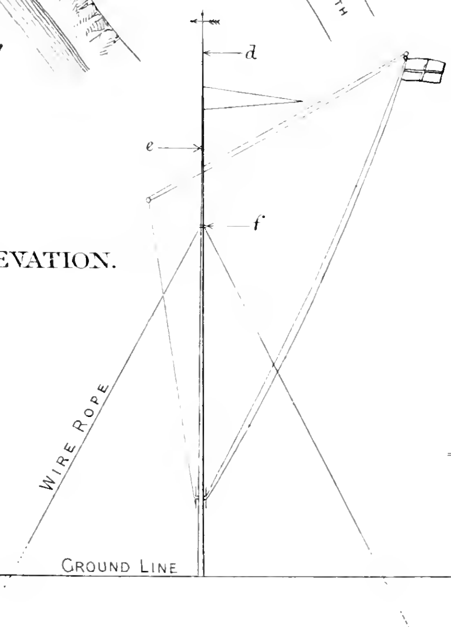
"I must now attempt to describe the effects on ourselves and the impressions on our senses, though I am conscious of difficulty in avoiding subjective matter here. Of the three, my wife only was 'struck,' and fell to the ground, my son and myself remaining erect, and all three retaining consciousness. For more than half an hour my wife lost the use of her lower limbs and left hand, both of which became rigid. From the feet to the knees she was splashed with rose-coloured tree-like marks, branching upwards, while a large tree-like mark, with six principal branches diverging from a common centre, thirteen inches in its largest diameter, and

Plan.





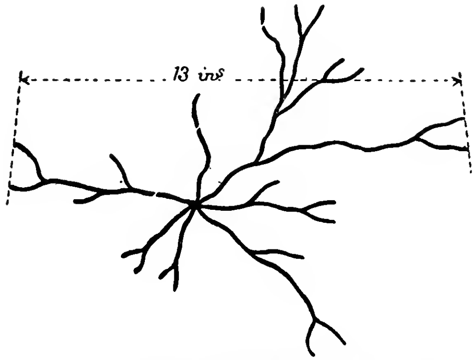
BROKEN LINK. FULL SIZE.



Elevation.

Scale, 1/16″ to a foot.

bright rose-red, covered the body. None of us are certain of having seen the flash, and my wife is sure she saw nothing. As to



Fac-simile of chief mark made by the discharge on Mrs. Pidgeon—from careful measurement.

the noise, my wife heard a 'bellowing' sound and a 'squish,' recalling fireworks; my son also heard a 'bellow,' while I seemed conscious of a sharp explosion. My wife describes her feeling as that of 'dying away gently into darkness,' and being roused by a tremendous blow on the body, where the chief mark was afterwards found. My son and myself were conscious of a sudden and terrific general disturbance, and he affirms that he received a severe and distinctly electrical shock in both legs. My left arm, shoulder, and throat especially suffered violent disturbance, but I did not think it was electrical. As I turned to help my wife, who was on the ground, I shouted, as I thought, that I was unhurt, and hoped they were also, but it seems I only uttered inarticulate sounds, and my son, in his first attempt to answer, did the same. This, however, was only momentary; in an instant we both spoke plainly.

"Neither of us referred the occurrence immediately to its true cause, but the idea of being fired at was present to all our minds; my wife indeed remained of opinion that she was shot through the body, until she heard me speak of lightning. An infinitesimal lapse of time enabled my son and myself to recognise lightning; but I cannot say whether I did so before or after my first glimpse of the wreck on the ground. Neither of us heard or saw the mast

fall, though it descended fifty feet, and fell on hard gravel close to us. My son and myself both experienced a momentary feeling of intense anger against some 'person or persons unknown,' further showing that we primarily referred the shock to some conscious agency. I ought perhaps to add, that neither of us felt any sensation of fear at the time; but we were all very nervous for several days after.

"I have endeavoured to keep to fact throughout, but I venture to add a remark made by my wife as we raised her from the ground: 'I feel quite sure that death from lightning must be absolutely painless;' and I offer it as an unconscious corroboration of views on this subject which our experience seems to strengthen.

"Though no electrician, I conclude from the splash of gravel on the garden-roller that the discharge was from cloud to earth, and the oxidised mooring-chains being inadequate to carry it all to ground, my wife formed a conductor for one of many sprays flying in all directions from the broken links."

Now (continued Mr. Preece) as this statement of Mr. Pidgeon was so clear and interesting, and as it answered several questions in connection with lightning which required elucidation, I placed myself in communication with Mr. Pidgeon, and he added some interesting facts to the information he had already given. I will just read one or two extracts from the subsequent correspondence.

"The sky," he says, "was 'wild,' but bright at intervals, one of those days which abound in beautiful atmospheric effects. Windgalls had been frequent, and scuds crossed the bay now and again. We were on the shore watching the exquisite play of colouring over the bay when a scud approached us rapidly.

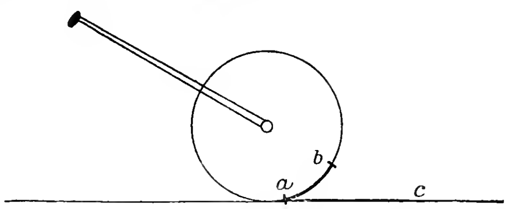
"We could see it coming four miles off, and as it neared, fearing a wetting, we ascended into the grounds by the steps shown in the plate, with a view to going indoors.

"On arriving at the positions shown at L, R, D, the scud proved to be hail, not rain, so we remained in partial shelter, still watching the effects over the bay. I cannot but imagine that the presence of our 50 ft. mast was, as you express it, 'the last straw which broke the camel's back,' and that the cloud might have passed without any restoration of the electric equilibrium but for it. It

had travelled in our sight many miles without discharge, but the moment it passed over the staff discharge took place.

"From the accounts of our servants in the house, our neighbours, the boatmen and coastguard officers, I should say the discharge was such an one as you characterise as peculiar to the masts of ships, distinguished from a mass of building. My friend and neighbour Colonel Fagan, an old Indian soldier who has seen much service, says it passed all his previous experience. His house is 500 yards from the staff, and he 'ducked to the shot,' to use his own expression, when the discharge took place.

"In the diagram (facing page 268) A shows the garden roller and B, B, B the sinuous furrows traced in the gravel. C is an iron drain grating, to which also short furrows were dug. The roller stood on the gravel path, the 'splash' on it was found as shown in the sketch below, where *c* represents the furrow and *a b* the splash: the ground was damp from the morning's heavy



rain, and no doubt the point at which the roller rested would be more damp still. The appearance of the splash might very well be imitated by supposing that a drop of water, charged with dirt and in the spheroidal condition, had suddenly burst into steam and scattered its suspended dirt over the circumference of the roller. The subsoil below the gravel path and turf is new red sandstone conglomerate.

"In the elevation *e* shows the place where the mast broke off; at this point it was spliced with the two iron collars. *f* is the point of attachment of the iron wire guy-ropes. The dotted lines at the ground end of the guy-ropes represent the chains in which they terminated. With regard to the blackening of the mast, this extended from *d* to *f*: the mast, which is twenty or thirty years old, was thickly painted with many coats of white lead. I presume the

discolouration is reduced lead. It was lucky the staff broke at *e* instead of *f*, because the heavy gaff did not fall.

"I have shown the peculiar double fracture of one link full size in the diagram; many were broken in this way, *and the fractures were more numerous through the sound than through the oxidised parts of the links notwithstanding their diminished sectional area.*

"I shall be very happy to answer any other questions you may ask, and very glad to get any complete explanation of the whole phenomena.

"I had almost forgotten to mention that my wife, standing at L, had just closed the low door leading to the shore and was looking over it out to sea. The iron bolt which fastens this door is *exactly* the same height from the ground line as the mark on my wife's body. The three broken wire ropes are marked O in plan. My son stood at R. I stood at D.

"One more point. *All* our servants in the house, my friend Colonel Fagan, his wife and daughter (house 500 yards from the staff), my son's tutor, and a young man reading with him, the boatmen and coastguard, all within 600 or 800 yards, say they saw a 'ball of fire.'

"People who were looking out of south windows when the discharge could only be directly seen from north windows, made the same remark, and a lady in Colonel Fagan's house, who was searching for something in a cupboard, observed the same thing. A most clear case, I think, of optical delusion this in respect of 'ball lightning.'"

Another point is, Mr. Pidgeon has brought the links themselves which were fractured.

Now there are two or three interesting points in connection with this discharge which corroborate some opinions expressed in this room and some of our theories on the matter. In the first place you observe, this high commanding tract, standing in the centre of the beautiful bay of Torquay, was the first object which this charged cloud would meet, and it was at that point the practical discharge took place with the effects described. Again, the discharge took

place from the vane to the points where the ropes joined on to the mast, and from that point to the ground the mast remained untouched—the same effect which we have experienced in telegraph poles; but we know with the passage of the discharge a peculiar phenomenon occurred in the chain which was most mysterious and which requires consideration. These links were broken, not broken by fusion, not by any ordinary electrical effect, but torn asunder by a sudden strain or stress brought on them by some mechanical means. Whether these mechanical means were due to electrical causes or mechanical rupture of the mast it is difficult to say. Mr. Pidgeon will, perhaps, be able to enlighten us on that point. Again, the discharge itself from the point where the lightning entered into the ground, sought, as we know electrical discharge will seek, the line of shortest resistance.

There is a curious fact connected with Torquay and Torbay, which I have mentioned before when discussing another subject, that is, that in these places we experienced great difficulty in finding good earth for our circuit. Mr. Pidgeon mentions that the stratum beneath his garden is red sandstone conglomerate. Torquay itself is built upon the pure limestone, but the whole is what we call insulated, so that we were obliged to carry plates out to sea a considerable distance before we could make good earth. At all events, however, the stratum underlying this garden was a bad conductor, because we see by the furrows on the ground that the discharge, in place of going direct to earth, rather flew over the surface of the garden path, moist from the rain. It tore up the gravel in deep furrows and then flew to the iron roller, making an extensive surface-contact with the ground, which was the best earth-plate it could find. There are one or two other points I should like to have mentioned; but I will now say one or two words on the subject generally. The subject of lightning conductors, as some of you know, is rather a hobby of mine, and when I do go into country towns and places, and have a few minutes to spare, I invariably go to the church, not only to admire the architecture, but especially to see what sort of lightning protector it is furnished with, and I am bound to say this—I have never been to one church

yet where the lightning conductor comes up to my notion of what a lightning protector should be. Sometimes it has no point, and even no earth. Generally it is made of the most expensive copper rod; sometimes of the most inefficient iron tubes, broken in the middle. In my experience of hundreds of churches, I have never seen one single lightning conductor that I would pass as a lightning conductor. I think this is a point which telegraph engineers ought to take up, because we, in our official duties, have occasion to go to distant places, and we are called upon frequently to go to church, and I am quite sure the telegraph engineer would be doing his duty, not only by going inside the church but by going outside and seeing how that edifice was protected from the effects of lightning.

There are two or three matters in connection with the conductor itself which have to be considered. In the first place, the point. The terminating of the conductor in a point is most essential. Colonel Stotherd has called attention to the metal of which the conductor is composed, and has raised a question whether it should be of iron or copper. He has given a description of the copper rods which are specified by the War Office, and, as I have said before, I think extravagantly. I think efficient lightning conductors can be obtained by the use of galvanised iron ropes.

Again, as to the earth. We have heard from Colonel Stotherd to-night a statement of the experience of the tanks at East London, at the Cape. Anything more sad to the electrician than to find a lightning conductor terminating in a tank lined with cement can scarcely be believed. I have only heard of one case to equal it. That was a case mentioned by Dr. Mann, where the proprietor of a house found the end of his lightning protector lying coiled in the basement floor, and he took it and carefully coiled it in a pail of water.

Another point which requires notice is the advisability of connecting with the lightning protector masses of metal which comprise the structure—such as galvanised iron roofs, iron tanks, pillars, and iron masses generally. I think all experienced telegraphists will agree in saying that to protect a house from lightning we ought to see that all masses of metal which compose the house

are properly connected with the lightning protector to protect it from those effects of "return shocks" which are as fatal and destructive as the direct discharge itself.

There is another point on which I had a correspondence not long ago with Major Majendie, who is the Inspector of the Home Office over powder magazines and the transport of powder. He states he had a correspondence with some gunpowder manufacturer as to the advisability of protecting his powder magazine from lightning, when the powder manufacturer endeavoured to prove to Major Majendie that lightning protectors in such cases were absolutely dangerous, and that his manufactory was safer without a lightning protector than with one. The argument adduced was that his buildings were surrounded with trees —trees themselves having a tendency to dissipate the charge. We all know that trees may conduct electricity, but they do not protect a building from electricity. We know when a tree is struck by lightning the branches are wrenched off, the bark is riven off, and the poor tree comes to grief. If the tree itself is not protected from lightning how can it serve as a protection for a building near to it?

An interesting point connected with the phenomena which occurred on the premises of Mr. Pidgeon is the form which the discharge of electricity is alleged to have taken. We have heard explanations of appearances called "fire-balls," and, as Mr. Pidgeon says in this particular case, some of the people who saw the flash described it as having the appearance of a ball. Another interesting point in the phenomena was this: Mrs. Pidgeon was at the time of the occurrence resting her arms on a gate, looking towards the sea, and when she was struck the flash, as I have read to you, made those marks upon her body—those tree-like marks. Now, in numerous cases where individuals have been struck by lightning, marks have appeared upon the body, and there are several instances on record, well authenticated, where the photographed images of objects have been marked on the person. There is one well-authenticated case, where a sailor was standing by the mast of a ship upon which a horse-shoe was nailed, and upon his breast there was a distinct representation of the horse-shoe. In other cases

people struck by lightning have had marks upon them, but for the most part they assume a tree-like form, which may be explained on the principle of the vein-system of the body; the nerves or veins become discoloured, and this leaves the tree-like marks. The photographed image is more difficult to account for, but no doubt the molecules which are disturbed by the force of the discharge take the same configuration as the object which they strike.

I repeat, that in using copper wire so freely for lightning conductors we indulge in extravagance. There is another reason why copper is objectionable. The copper wires are attached to a building by galvanised iron wall-eyes, and when a building gets damp we have all the conditions to generate a current. We have copper, galvanised iron, and moisture. The result is a current that produces electrolytic action, and I have found on examining conductors distinct traces of the electrolytic action tending to destroy the utility of the conductors. The subject is one of immense importance, and electricians cannot do better than apply their knowledge to the use of the community at large by seeing that the buildings which are open to them are properly protected from the dangerous effects of electricity.

Mr. PIDGEON said: Mr. Preece has made it necessary for me to do that which was far from my intention, viz., offer some remarks upon the phenomena he has brought before you. Having stated all the facts in my communication to *Nature*, and my subsequent letter to Mr. Preece, and being no electrician myself, my duty is properly at an end, and anything I have to say to-night must resolve itself into stating my wish to answer any questions which you as electricians may ask for the further elucidation of the phenomena. There is one remark I would make with reference to the fracture of the links, and I will lay them upon the table. These links, you will see, are broken fair across as if by a sudden and violent blow; not broken as if pulled out gradually. My first impression after the occurrence was that the mast had been forced bodily over by the discharge in some way which I did not understand; and thus, acting as a lever, had wrenched the links apart. If,

however, this had been the case, I should have expected that the links would have yielded slowly to the strain, and would have been drawn out instead of being broken sharp across. Again, the mast itself was stepped into a square wooden socket, which it closely fitted, and on examining the wooden socket and soil around it I could find no signs of the slightest disturbance. If these links had been broken by the mast acting as a lever there must have been disturbance of the soil at this spot. I should be glad to have some information as to the manner in which the chains were broken, and how the furrows were scored in the gravel-path, as described in my communication to *Nature*. If any gentleman has any question to ask in further elucidation of this matter I shall be happy to answer it as far as I am able to do so.

In answer to PRESIDENT: The wind was south-east, and the altitude of the house above the sea about forty feet.

Dr. MANN (responding to the President's invitation) said: The paper read by Col. Stotherd is one of great interest to me, inasmuch as East London is within about 200 miles of the spot where the chief of my practical experience with atmospheric electricity has been gained. I must say it was matter of surprise to me to hear that in East London we have had another instance of those extraordinary water-tight cisterns taking the place of earth contacts. It is but a repetition of the old well-known case of the accident to the lighthouse at Genoa in 1827, in which the same state of things existed as has been described in the paper. A similar condition seems to be present in a great part of the land which rises immediately to the south of the Lake of Constance. Every house in that district has from two to eight lightning conductors. There is, nevertheless, never a season in which large fires from lightning do not occur. The soil is a porous limestone, that becomes during a part of the year exhausted of water, and there is consequently entire absence of efficient earth-contacts for the lightning conductors. Ninety-nine cases out of a hundred of imperfect action of lightning conductors can be traced to defect in this particular. The second instance of accident illustrated in the meeting is one of exceeding interest. In this case the flag-staff had earth-contact

of a certain kind, or the discharge would not have passed down as it did, but the earth-contact was a bad one. The discharge was carried through the metal rope stays, and from them to the earth by a very large and diffuse bad earth-contact, which in this instance supplied the place of the well-arranged earth-contact, which would have carried away the discharge without mechanical mischief.

Regarding the effects upon Mr. Pidgeon and his companions, in all probability this was an illustration of the operation of the return shock. If the discharge itself of the lightning had passed through their bodies they would not have been alive now. Marks upon the body might be produced by secondary lateral discharge, resulting from inductive action, concentrated on the point of greatest energy. In this instance it is quite obvious that a very large amount of mechanical influence was exerted at the lower part of the stays of the masts. There were four short pieces of iron chain there, of which many of the links were broken. In one of these links, which has been placed in my hands, I perceive traces of what looks to me very much like imperfect molecular cohesion in the part where the disruption has been occasioned. The chief effect of the disruptive force was exerted as the electric discharge escaped through these points of increased resistance and different strength, producing a powerful mechanical effect of disintegration upon the several disconnected links of the chain. Where this disintegrating effect and molecular disturbance were the greatest, the metallic continuity gave way. It should also be remarked that it is well known that metallic rods are *shortened* by powerful discharges of lightning that do not suffice for their fusion. This condition brought about in the continuous rope stays very probably facilitated the disruption of the chain links at the weak part.

Professor Tyndall once received by accident the whole charge of the Royal Institution battery of 15 Leyden jars, and he states he was perfectly unconscious that any discharge passed through him. I incline very strongly to the opinion that Mr. Pidgeon and his companions experienced an electrical discharge of this character, and not stronger, either caused by the lateral inductive

action or by the very large diffusion of the discharge in all directions round as it accomplished its passage to the earth.

The fact of this secondary inductive disturbance is easily illustrated in various ways, but in no way more strikingly than by the simple experiment in which copper wire is coiled round upon itself many times, with the separate coils insulated in melted resin, so that when a secondary coil of exactly the same kind is placed within a quarter of an inch as a powerful electrical battery-discharge is passed through the first coil, a strong spark is seen developed in a small break of continuity in the other at the instant of the discharge.

We have, however, at present, no adequate idea of what the intense action is at the instant of discharge even in good lightning conductors. This is just one of the points which need further observation and investigation in our dealings with this practical application of electrical science.

The PRESIDENT: We have with us to-night Captain Stiffe, who has had considerable experience of storms in tropical climates. I would ask him whether he has arrived at any results or theory?

Captain STIFFE: My experience has been of a negative kind, as showing the perfect protection of the lightning conductors adopted by the Navy. I have never seen a ship struck by lightning. I have seen electrical discharges from all points, but the protection is so complete that it never comes to a discharge upon the ship in a storm.

Major MALCOLM, R.E.: I am going to be guilty of what I am afraid may seem to some to be a heresy. It is generally assumed that lightning conductors take the discharge from the cloud and carry it down to the earth: but I wish to ask whether Mr. Pidgeon's experience does not point rather to the fact of conductors taking the charge from the earth and discharging it towards the cloud? Our particular difficulty this evening is to account for the breaking of the links. Now if the discharge from the earth was carried up the wire ropes and there found a difficulty of travelling out by the point to the cloud, and in travelling split the upper portion of the mast, would not the energy which was expended in doing that cause such a severe shock upon the chain as very probably to break

the links? I know from experience when iron—even the best wrought iron—is suddenly broken it breaks with a crystalline fracture; so that it need not be assumed that anything except a very sudden shock caused the crystalline appearance of the fracture; and I think there is another point which shows that this was a discharge upwards: that is, the fragments of wood that were carried 150 yards distance. That looks almost like their being taken up into the air and carried away by the wind, and is not what one would anticipate if, as is ordinarily imagined, the lightning travelled downwards to earth.* If I had been aware of what was coming on to-night I would have asked a friend of mine who was struck by lightning—at least he received a blow on the head—to come here. One evening he was struck, seeing a great flash near him and sparks from the ribs of his umbrella, and the next morning he went back to the place, and found a large pit where the lightning had gone *into* the earth. I asked him what was the shape of the edge of the pit, and he told me the earth was all scattered about. That looks as if the discharge from which he suffered came out from the earth. As to the question of cemented tanks, if it is thought desirable to ascertain what their resistance is, there are some good ones at Shorncliffe. They are in a nasty dry soil, but I can answer for their being watertight—I made them myself.

Mr. Graves: With regard to one or two of the points raised, I think I may make contribution of the facts within my own experience. The general question of insufficiency of lightning conductors, the reason why they fail to conduct, and the fractures that are found to exist, resolve themselves into one of two causes; either the material of the conductor is insufficient or the earth connection is not properly made; good earth is not provided. I may mention, as bearing upon the latter point, that some years ago I was engaged at Stockport, which lies almost entirely upon the new red sand-

* Between the night when I said this and my words going to the printer, I have noticed in the papers several instances of lightning being said to descend; in one case a chimney, in another through the roof, and the popular notion seems to be that a flash of lightning is a thing like a butterfly, which can be caught by a lightning conductor and pocketed; or perhaps, as damage is done, like some sort of cannon-ball which can in some mysterious way be caught and got rid of.

stone. I experienced some difficulty in obtaining earth for railway purposes in connection with the electrical block apparatus. It was impossible to find what is called good earth; the current escaping from one wire to the earth reappeared on other wires terminating at the same points. We carried the wires from the instruments into the river which runs through Stockport, but the result was very little better; the earth was somewhat improved, but still imperfect. In the end we had to run main-line wires $2\frac{1}{2}$ miles back, and then connect them with an earth-plate, after which we succeeded in our object.

About a year ago, dining with a millowner in Stockport, the conversation turned upon lightning conductors. I told him the difficulty I had experienced in some neighbourhoods of getting earth for ordinary electrical connection. I asked him whether any attempt had been made to ascertain in the case of the lightning conductor of his mill, whether good earth was found, and he told me that all he knew was, that a copper-wire was fixed against the chimney-wall with its end planted some five feet deep in the earth. I made other enquiries on the same subject, and in one case I made a test of the conductor, and found the earth resistance was considerably over 1,000 ohms, it being evident that no endeavour had been made to ascertain the nature of the earth obtained. Lightning conductors are put up in the majority of cases carelessly, without regard to the resistances they meet with, and once put up are left entirely uncared for. The conductor is supposed to do its duty year after year without any attention to its maintenance. Mr. Preece alluded to his experience in the examination of churches, and I know myself of a case, that of Llandaff Cathedral, where the conductor is fixed to the spire of the church, without any earth connection existing whatever. The conductor had been fixed to the building, but it did not appear to be within the province of any one to see that it was capable of doing its duty. This absence of inquiry into the nature of the earth made, and the complete absence of attention to conductors when once fixed, lead, I am certain, to a great popular delusion as to the amount of protection which is afforded to many of our public buildings against the risks of lightning.

Dr. Mann: Allow me to interpose one further observation. I think we are somewhat unhappy in our constant application of the terms "negative" and "positive," because they seem to imply a movement of the meteor in one direction only in a lightning discharge, whereas in the case of electrical discharges there is invariably transmission of the force in both directions. When a Leyden jar is discharged through a card it is notorious that there is a burr of outwardly-raised edges on both sides. Mons. Callaud alludes very pointedly to this fact in reference to discharges of lightning, and he says, "Le ruban de feu qui unit le nuage à la terre va aussi de la terre au nuage." Again, with regard to the actual movements of earth, wood, or other material substance at the instant of a lightning discharge, that movement is entirely the result of the secondary mechanical force developed by the passage of the discharge amongst material molecules. It is essentially similar to the explosive scattering of material substance by the sudden generation of steam in confined spaces. The terms "ascending" and "descending" lightning are only admissible when they are intended to express the neutralization of the effects of an influence which in itself is no more an actual transportation onward than a wave is the transportation onwards of the water that undulates.

Mr. Kempe: It is stated that unnecessary expense is incurred in using copper in the place of iron conductors. There are one or two points to be considered in this assumption. It is true that copper conducts better than iron, but is not the question this: Should we not use a thinner copper conductor than is usually employed? I think that in that case, regard being had to the higher conductivity of the one metal over the other, the cost would be in favour of the copper. The great objection to iron is its liability to corrosion. Copper is free from that defect. If you use thin copper wire in the place of thick iron wire, it might be argued that it would be susceptible of mechanical injury. We must recollect, however, that conductors can be carried inside as well as outside buildings, therefore this objection falls to the ground. The question then resolves itself into this: Is it not better to employ

copper wire of smaller dimension than that which is generally used at a less cost, than to use a larger diameter of iron-wire at a greater cost?

Mr. SPAGNOLETTI: I had the pleasure of a conversation with a scientific friend at Weymouth, and he pointed out to me the different effects of storm clouds very distinctly. He said, so distinctly were they visible, that he could watch the effects from his windows. He noticed, when a damp south-west cloud was blowing in the upper regions, and a north-east dry wind on the earth, you get the effect of a Leyden jar, the dry east wind acting as a substitute for the glass between the two electrified bodies; and he particularly noticed the effect of the discharge. The lower end of the cloud generally discharges to the earth, while there is a corresponding discharge from the earth into the cloud. There is another curious thing with regard to the passage of electricity respecting the Atlantic Cable. I was told by the Chairman of the Great Western Railway Company, that on one occasion a message was sent from France on the French cable to America, and the same message was received at Valentia, showing an effect as if there was a contact between the two cables, although they are fifty miles apart from each other. I asked what was the formation of the bottom of the sea, and was told rock. I think from that it is tolerably clear that the resistance of fifty miles of salt water must have been less than the resistance of the rock. With regard to the effects of lightning upon persons struck by it, I may mention that on the Shrewsbury and Hereford Railway one of our linemen was engaged up amongst the wires at the time a storm was going on at Hereford, fifty miles off. The lightning struck the wires at Hereford, and travelled to Shrewsbury. The man was struck, and was burnt in the arm and side, between which he had a wire, and in the leg by another wire, by which he was supporting himself; he was rendered senseless, and luckily caught his foot in the wires, by which he was suspended until removed. He was very ill for about a month, but although he resumed his duty he was never the same man, and died a few months after. With regard to the difficulty of getting good earth, I have found that to be the case in South Wales, and rocky

districts, particularly in the Swansea Valley, and to obtain a good earth there I was obliged to run the wires out a considerable distance from the Railway, and to give a separate earth to each instrument. The rails on a line make an excellent earth or return channel, but the interference by renewing rails and packing are drawbacks to their use. I quite agree with Mr. Preece and Mr. Graves as to the neglected state of lightning conductors to buildings and structures generally; professional aid in these constructions is evidently badly wanted. The three chief points of a conductor should be a sufficient conductor, good earth, and good joints where required. It is well to have them insulated from the buildings they are intended to protect. Copper wire is the cheapest and best conductor that can be obtained, and if it is a temptation to the thief it might be joined to an iron cable five times its sectional area, at a distance out of reach; a good plumber's joint should be made at the point of junction. Good earth as a rule will always be found at the yard-pump, the pipe leading to the well, or, if the wire itself is led into the well, it will be sufficient; low, damp, and clay soils are generally good, and a hole five or six feet deep in such ground will be found all that is necessary.

Major MALCOLM, R.E.: I would wish to ask Dr. Mann how he accounts for the splash of gravel upon the iron garden-roller. Mr. Pidgeon has likened it to the bursting of a drop of water. Also, how he accounts for the channels cut in the ground; the diagram shows that they were of considerable extent. I think there must have been some mechanical motion in the action of the discharge. I have been informed that in 1848 or 1849 the band of the Royal Artillery were playing on Woolwich Common, at a place known as the Seven Sisters' trees. A flash of lightning came and struck several of the brass instruments out of the men's hands, on the south side of the circle. It also struck three men on guard some yards off, on the north side. Two of these men were killed, and the third was injured. These men were several yards apart from each other, and wore side-arms only. One of the men's bayonets was struck upwards and injured the man's chest, while the bayonet was twisted into a corkscrew shape, and this is still preserved in the Repository

at Woolwich. There must have been mechanical motion somewhere to do that.

Col. Stotherd: There are one or two points to which reference has been made which call for notice. First, as to the size of the copper conductor: the late Sir W. Snow Harris recommended larger dimensions for lightning conductors than those we employ at present. He also suggested the use of iron rods, and tubes and bands of proportionately large sectional area. We have recently reduced the dimensions recommended by him to those mentioned in this paper. Many eminent authorities think that these dimensions might be still more reduced; but where the building to be protected contains dangerous explosive substances we consider it better to err on the safe side. Second, as to the connection of metal used in the construction of a building. The rule with regard to Government buildings, including powder magazines, is, that all metal shall be put in connection with the system of lightning conductors. Third, with regard to the question of trees round a magazine, the case referred to by Mr. Preece was explained to me, and my opinion asked, whether the trees, which were much higher than the building itself, would not be a sufficient protection without any lightning conductors of ordinary character. The opinion I gave was identical with that which has been mentioned, and on which I believe Major Majendie acted. Fourth, with regard to tanks: we no longer use them for the earth connections of lightning conductors. Years ago there was an order that lightning conductors were always to be led into tanks—the idea being, one would suppose, that the water would put the lightning out; but we have done away with that; tanks are not now used, and I think I may say we adopt more scientific principles. The powder magazine at the Cape, referred to in this paper, was built about twenty years ago, when this order was in force; but now we always lead the metallic conductor into moist earth. Fifth, with regard to the relative cost of copper and iron: my experience is, for an equal expenditure of money you can produce something like an equal amount of protection with iron as with copper; that is, with iron you can have a greater number of conductors, and perhaps a larger

number of iron conductors distributed over the same surface may be better in certain cases than a smaller number of copper conductors. Where the effect of an explosion would be attended with great danger to life and damage to property, the question of expense should not be considered of primary importance. Sixth, as regards the liability of iron to rust and decay, we purpose to use galvanised iron, and if the galvanising is well done, it will last for a considerable time. I think the selection of the metal to be employed must be left in the hands of the constructor, who should take all circumstances into consideration. For example, near chemical works, or where acid fumes are encountered, the most durable metal would not only be most permanently efficient, but in the long run most economical.

The PRESIDENT: We have had a very interesting discussion, and I would in the first place bear testimony to the clear description which Mr. Pidgeon has given of the effects of that stroke of lightning; perfectly scientific and perfectly modest, it is a model of what descriptions of natural phenomena should be. Any one who has amused himself with a Leyden jar will be struck with the difficulty of retaining a small quantity of electricity in ordinary places. Yet when we come to use low tension in electricity we find in our experience that large portions of the earth's surface are very bad conductors indeed, and nothing is more usual than to meet with instances such as have been adduced, of cases in which with a very minute quantity of electricity from ordinary batteries you can trace the current at a distance of a mile or a mile and a-half from the point at which it enters the earth, showing what a bad conductor the earth often is. With regard to the case mentioned to-night, in which a signal sent by the French cable was read on the instrument at Valentia, I may mention that there is great difficulty to prevent a signal sent to St. Pierre from being read by anyone else who carries a telegraph there. I have very little doubt myself that the cause of the apparent contact mentioned by Mr. Spagnoletti was, that the signals were sent from Brest to St. Pierre and thence to Heart's Content, and then came back to Valentia. Now there have been some curious cases mentioned

this evening. The snapping of these links is a puzzling phenomenon, of which no one has given a satisfactory explanation. I have none to offer myself. It is a curious fact, that when an electrical current passes through a bad conductor it has a different effect to when it passes through a good conductor. If you send a discharge through gunpowder from a Leyden jar it does not fire it but blows it away, but if you pass it through a bad conductor, such as a piece of wet string, that gives time to ignite the powder and it goes off. So in the case of this phenomenon which has been described to-night. Evidently the whole of that piece of ground is a bad conducting medium, and the time it took for the discharge to pass off produced the terrible effects of which we have heard. So, in the case of the tanks at East London, there seems to have been the same thing. Colonel Stotherd describes it as a discharge carried up with disruptive effect, and so, when we meet with a bad conductor, we have all sorts of disruptive effects, but where you have a good conductor we have no such effects.

With regard to the question of iron *versus* copper, no doubt the conducting power of copper is some five or six times greater than that of iron, and the price, roughly speaking, is in about the same proportion; so that, price for price, you have the choice of materials, and it is for the electrical engineer to judge whether he shall use a pound's worth of copper or a pound's worth of galvanised iron. The only objection to copper is its liability to be stolen, and I have known that to occur to a considerable extent. With iron we have no such fear.

It is universally accepted by all electricians that lightning conductors should be carried well into the earth. I am not disposed to gainsay that, but if the lightning conductor is continuous through the whole length of the building you cannot shelter the building without producing some disturbance at the base of the conductor, for as long as the conductor is entire the building is preserved, though you make an imperfect connection at the base. It is probable that only a portion of the electricity will return through the other portion of the building if the conductor is in itself a good one. A bad lightning conductor, however, is

better than none. I do not make these remarks with a view to recommending any sort of conductor; on the contrary, I think we ought all to point out to those with whom we come in contact the very serious results which may ensue from the use of bad lightning conductors. Again, regarding large masses of metal, it is easy to imagine that the disruptive effect between large masses of metal and a good lightning conductor might be very dangerous. Judging from the surface of an ordinary mass of metal, such as bells, I am not of opinion that any serious shock would pass from a peal of bells in a steeple through a lightning conductor; and I do not consider it is so important in that case, as in some others, to make connection with all the mass of metal about a building. I think if you conduct from the exposed projecting portion of the building, and make good connection with moist earth, it is sufficient.

Now, in this case in point there is something very interesting in the tearing up of the earth, supposing the attempt of the shock to be to get to the iron roller and so pass off in that direction. I do not think sufficient use is made of the lead spouts and other leaden connections about a building, and I am of opinion that if lightning conductors were put up by electricians they might make more use of the leaden spouts and leaden roofs of the building. My own house has been more than once struck by lightning. I have only the ordinary arrangement of the lightning conductor in connection with the ordinary water spouts, the lead water pipes, and iron gas pipes; with large iron pipes I conceive there is no danger. The house stands on a very elevated position, and, although it has received numerous shocks, no injury has resulted. On one occasion a shock of lightning fell at my house. It struck the lightning conductor and a flag-staff about 35 yards distant, and it also struck a water tower rather higher than the house in the opposite direction, distant about 22 yards, which also had a lightning conductor, consisting of a 15-inch iron pipe with a spike on the top. It struck all these three objects at the same time. My wife saw the lightning run down in a zig-zag direction, and she had no idea the house had been struck; but it went to a lofty beech tree, which stood in my grounds rather higher than the house, and, forcing itself down to the

ground, a portion of it passed through an ordinary fishing landing-net with a cane handle. It completely split the handle into ribbons and made several holes round the base of the tree. It passed down this wet cane handle in sufficient quantity to make a large hole in the ground, but it did seem to not injure the beech tree; but since the occurrence a large portion of the bark has peeled off. When I was in the Persian Gulf I witnessed some storms which were very interesting. We were in ordinary storms protected by the usual means employed at sea. I should say, usually the lightning appeared to fall from the sky to the sea; in some instances it appeared to rise up and out of the sea and pass upwards. Although these storms lasted for several minutes, it is curious that although the cable was being paid out, and the connection between it and the ship was made by a delicate Thomson's galvanometer, we could not distinguish the slightest tremour of the galvanometer during any part of the storm.

The meeting then adjourned.

LETTER IN CONTINUATION OF THE SUBJECT OF THE MEETING, 12*th May*, 1875.

It has occurred to me that I might, perhaps with advantage, attempt to make more clear the meaning of the few words I addressed to our Society the other night in connection with the subject of lightning conductors, especially as I found objection taken by one speaker to expressions used colloquially, expressions which I am afraid must, in the present state of electrical knowledge, be used from time to time, though they will always expose the user to cavil.

I said I was going to advance what I feared would be received as heretical doctrine, and my meaning was simply this. Whereas many preceding speakers had spoken in a sense which I understood to mean that the use of lightning conductors was to convey the lightning stroke or flash to the earth, there to lose itself,—and whereas they also seemed to think that if any obstacle intervened

to prevent this flash which came out of the clouds from passing by a perfect path to "earth," damage ensued from the interruption of the lightning's peaceful progress,—I wished to suggest that the true view of the duties of a lightning conductor was rather that it enabled that portion of the surface of the earth with which it was in connection to relieve itself of the tension induced upon it, and that in fact, could an ideally perfect system of lightning conductors be erected, a lightning flash would never be seen within the area protected.

I am conscious that I failed to express myself so as to be understood except by a few. Subsequent conversation showed me, however, that there were some who understood me in the sense in which I wished to be understood, which I will repeat, viz.:

The real office of a lightning conductor or protector is to relieve the tension or strain (electric) of that portion of the earth's surface with which it is in contact, by providing a point through (or from) which the earth's tension (or potential) can be relieved by discharge in the direction of the inducing cloud.

This I put in direct opposition to what I believe to be the current opinion which imagines the conductor in some way catching hold of the lightning and conducting it safely to earth; and it is this opinion, more or less unconsciously held, I believe, which caused us to hear so much of the *return* stroke.

If a flash of lightning be an electrical discharge, it is surely an effort of nature in the direction of equilibrium, out of which I cannot see that fresh disturbance is to arise; in short, I cannot believe in return strokes, but I can believe in picturing to myself and in many ways reproduce the following conditions of things, viz.:

A  B

Two conducting bodies, A and B, separated by a non-conducting layer, or dielectric.

One of these conducting bodies, A, becoming charged, charges the opposing surface, B, by induction, and, speaking roughly, the charge is equal and opposite to the original charge.

If now B be a perfect conductor, and I manage to place a perfect pointed conductor C upon it, making good contact, C pointing in the direction of A, *e.g.* stick a pin on the plate B, the conditions

become modified as follows: viz. in the first place the thickness of the dielectric is reduced; in the second, by known effect of points, the electric density of the system B C is greatest at the point C, and dissipation takes place first from that point towards a corresponding point C in A; but more than this, as has been asserted by Sir W. Thomson, and as some of my experiments published in our Journal go to prove (which are supported by others, not yet published, by Mr. W. H. Preece,) the resisting power of dielectric substances fails them in proportion to the electric tension or pressure they are called upon to resist, therefore by establishing this point of pressure C, we also, as it were, establish a line of weakness or even we may say a leak in our condenser along the line C C.

Many of us, I dare say, worked with condensers that were not "tight," and they can appreciate my view of a lightning conductor which does its work.

But of course if we put a leaky condenser near a tight one, it does not follow that the tight one shall be effected by the leaks in the other, so it seems to me that the leak established by a properly working lightning conductor cannot be expected to extend over an indefinite surface of the earth; and again, nature may not wait to admit of this silent discharge, which I picture to myself would only take place under the conditions of perfection with which I started; those who do me the honour to read this will easily apply the limitations.

With regard to my attempted solutions of the mystery of the broken links, it may not be altogether out of place for me to remark that the expression made use of in the course of the night, "mere disruptive discharge," cannot carry the weight which was sought to be laid upon it.

Disruption or any other discharge must take place from a point or surface of pressure to one of less pressure or of comparative relief.

Any one accustomed to use an electric light or vacuum tubes must be well aware that somehow or other there is an actual mechanical transport of matter from one pole to the other; and

those who will think upon these things will not, I hope, be long in coming to the conclusion at which I arrived, viz., that the links were broken by the jar caused by the explosion of the upper part of the mast.

The blow which affected Mr. Pidgeon and family I account for by their being made use of to relieve the tension of the earth at the critical moment, though whether it would have been greater or less had they been better conductors I should not like to assert.

If my views are right, they point to connection with the earth being of the highest importance, so that electrical strain may be relieved.

If good earth connections are not made, the relief and protection afforded by a lightning conductor must evidently fall almost to insignificance.

One result, which it appears to me might reasonably follow from an intelligent comprehension of the nature and action of lightning conductors, is, that the hideous spike at the top of the Duke of York's Column might be so far cut down as no longer to be offensive—'tis but a little matter, but offends the eye.

C. W. MALCOLM, Major R.E.

ORIGINAL COMMUNICATIONS.

FALSE DISCHARGE.

By G. E. Preece.

The following experiments were undertaken to verify some statements relative to the false discharge obtained from *coiled* core or cable.

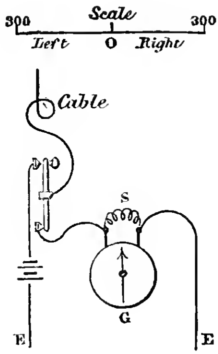
Experiment 1.—One mile of core, 107 lbs. copper and 150 lbs. gutta-percha, wound neatly round an iron bobbin, with iron core, and also iron flanges. The bobbin (ready for serving and worming machine) was placed with two others (precisely similar) in a circular iron tank nearly full of water; the gutta-percha wire was entirely immersed—the upper flanges of the bobbin being above water for a few inches only.

From the two ends of the coil two wires were led direct into the testing-room for facility of operations. The several tests were simply those of discharge, with the extreme end *free*, and with it to earth.

From these connections, it will be seen that by pressing the key K down, the core or cable was charged for any stated period; releasing the key it pressed upwards, discharging the core through galvanometer and shunt to earth. The galvanometer connections were so arranged that the discharge when the opposite end of core or cable was free was invariably to the left or to the side of the observer.

Experiment 1.—1 mile of core on iron bobbins. End free. Discharge with 10 cells = 4,120°.

No discharge to L, but so great to R that 10,000 shunt would not bring it on scale.



1 cell on same core. End free. Discharge = 412°.

End to E. No discharge to L, but discharge to R = 85° × 10,000 S = 850,000°, or as 2,700 to 1.

Kept a permanent current of 10 cells on core with end to E.

Found bobbin highly magnetic, tried with common screwdriver, one flange attracting, the other repelling.

Adjacent bobbin in tank also magnetic.

Experiment 2.—One knot of precisely similar core, but, instead of being wound round an iron bobbin placed in a loose bundle or coil in water inside a square iron tank, immersed in water.

One cell:

	Discharge	end free . . .	333° to L.
	,,	end to E . . .	0° to L.
1 cell	,,	,, ...	2° R.
10 cells	,,	,, ...	5° R.
100 cells	,,	,, ...	15° R.

Experiment 3.—Working section of 7-wire cable, 9 knots.

5·5 knots were sheathed.

3·5 ,, served only.

1 cell—Discharge end free = 3,400 L.

,, ,, end to E = 2,400 R, nil L.

Experiment 4.—Completed section of 7-wire cable, 52 knots:

1 coil—Discharge end free = 21,000 L.

" " end to E = 35,000 R, nil to L.

In this, deflection varied considerably at times, due probably to testing going on elsewhere. No discharge whatever was observable in the left or proper direction, but invariably to right. A peculiarity showed itself with this experiment, in that the discharge (false) was at first sluggish, and suddenly, at about middle distance, received an impulse which carried it to the completion of throw.

A difficulty was caused by the proximity of the wire to the instrument causing a deflection in the latter from its great sensitiveness.

From the foregoing experiments it will be seen, that, when an insulated wire is made into a coil, the discharge obtained from it when its opposite end is to E is the reverse indication of that obtained from the same coil when its end is free.

In each case no discharge whatever is seen in the same direction.

1. This false discharge appears least in a coil unsheathed placed within metallic or inductive points.

2. It appears greater when the same coil is placed within (for the whole of its length) a metallic sheathing, offering inductive or magnetic points without its entire length instead of without its mass.

3. It appears greatest when the coil is wound round an iron core. Under these circumstances the discharge is most powerful, and the iron core becomes magnetic. This form simply represents an electro-magnet, or the core and primary wire of an induction coil; the powerful "false discharge" being the same as the "extra current" of the primary circuit (Culley, p. 71, par. 145) heightened by the magnetic influence of the core.

a. If an insulated wire be made into a coil, be stretched at length, or be sheathed and coiled, or, so sheathed, be stretched at length, the discharge obtained from it with its opposite end free is invariably in the same direction, and is also of the same amount; the approach of the iron wires to the core apparently does not increase the inductive capacity of the coil.

b. If the same coil be stretched at length, sheathed or unsheathed, and its opposite end be put to earth, the discharge is also in the same as *a*, but naturally of very small amount.

c. If, again, the same insulated wire be made into a coil (1), be coiled round an iron bobbin or iron coil (3), be sheathed and coiled (2), and its opposite end be put to earth, *then* the discharge appears in the opposite direction, or the same direction as the *charge*. It is least in 1, greater in 2, and greatest in 3.

The cause is to be seen in the induction of coil upon coil, producing in each case an *extra current*. This is seen purely in case 1. This induction is heightened in case 2 by the magnetisation of the iron sheathing increasing the induced current. That it is not due to the extra inductive effect of the juxtaposition of the iron wires is proved by the fact that the inductive capacity of a coil is the same when sheathed as it is before sheathing. In case 3, again, the extra current is increased to its greatest extent by the whole of the coil acting directly upon the iron case, rendering it magnetic, and consequently which comes into play in inducing magneto-electric currents in the wire.

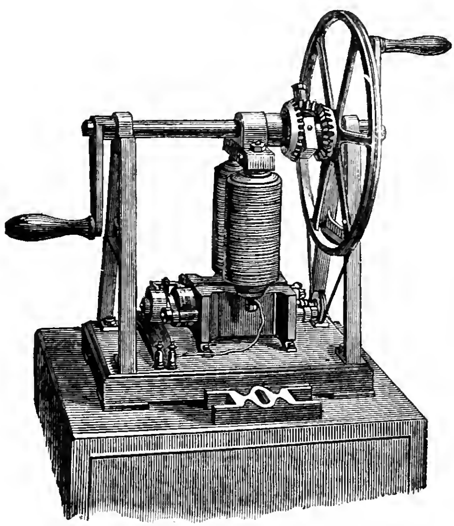
It is therefore evident that "false discharge" is primarily the effect of the inductive action of one coil upon another, and also that it is heightened by magnetic effect, depending for the greatness of its effect upon whether the coil be within or without the iron to be magnetised.

That this is so, the experiments of a coil surrounded with an iron mass, and a similar coil surrounding an iron mass, conclusively prove.

ON A NEW FORM OF DYNAMO-MAGNETO ELECTRIC MACHINE.

By S. C. Tisley.

In the first machines constructed by Siemens and Wheatstone in 1867 (see Royal Society's Transactions) the power of augmenting the magnetism by successive currents developed from the original residual magnetism contained in the iron was fully demonstrated, and it was shown that the power of the machine could thereby be developed to a great extent, but the only means for obtaining external work was by the insertion in the circuit of a magnet or coil, so that the secondary discharge could be utilised. Sir Charles Wheatstone also showed that a great part of the current could be shunted through a platinum wire, care being taken that the resistance of the platinum wire was sufficient to compel a large portion of the current to pass round the electro-magnet.



In the same year the writer designed a machine, which was made by Mr. Ladd, and described by him in a paper read before

the Royal Society (see Transactions), the principle of which was, that, two separate armatures being introduced, one was employed for magnetising the machine, the other being used for external work. This machine gave a good electric light, &c., and was shown in the Exhibition of Paris, 1867, where a silver medal was awarded for it. To simplify this machine the author of this paper afterwards placed the two armatures in the same groove, between the poles of the electro-magnet, bolting the two together at right angles to each other, so that they came under the influence of the magnetism alternately; by this method one pair of bearings was sufficient instead of two, and the machine altogether was much simplified.

The machine now about to be described is a still further modification, in which the greatest amount of simplicity and effective power are combined.

The apparatus consists essentially of an electro-magnet, with shoes forming a groove, in which a Siemens armature is made to revolve. This is much the same as the original machines made by Siemens and Wheatstone, but the difference occurs in the break or commutator, where there are two springs or rubbers employed, taking the currents off from the commutator. The commutator consists of three rings—one of these rings is complete for three quarters of the circle, the other quarter being cut away. Another ring is cut away three quarters, leaving the one quarter. In between these two rings is a third ring, insulated and connected with the insulated end of the wire wound round the armature; on this centre ring are projecting pieces, one a quarter of a circle, and the other three quarters, so arranged as to complete the two outer circles. The rubber spring which comes in contact with the quarter of the middle circle is connected with the electro-magnet of the machine, and the armature is so arranged that at the time of contact the best magnetising current is developed: the other spring-rubber is in connection with the wire on the armature during the other three quarters of its revolution, and this is connected with any external piece of apparatus required to be worked. By this arrangement, the alternate currents being utilised, they are all in

the same direction, and by the length of contact the whole of the current is obtained in the best condition for heating wires, decomposing water, giving an electric light, and other usual experiments.

At present a model machine has been constructed on this principle, the armature of which measures 5 in. long by 2 in. external diameter, on which is wound about 50 ft. of cotton-covered copper wire, No. 16 B. W. G.

The magnet has about 300 ft. of covered copper wire, No. 14 B. W. G. The whole instrument, without the driving gear, weighs 26 lbs. With this apparatus 8 in. of platinum wire, ·005, can be made red hot, water is rapidly decomposed, &c., &c.

The armature is constructed specially to prevent the accumulation of heat, to which every class of dynamo machine is liable.

It is made in two halves, a groove of a zig-zag shape being cast in each half, so that when the two are screwed together a continuous channel is maintained through the bearings for a current of cold water to pass during the whole time the machine is at work.

The advantages suggested by these arrangements are their extreme simplicity, the few number of parts, only one armature and one wire being used.

This principle of the alternate currents being utilised is also applicable to machines constructed on the multiple armature principle, and the economy thereby resulting would prove of great advantage, as the power of the machine could be varied by throwing into the electro-magnet either every other current or every fourth, sixth, or eighth current, according to the strength required in the machine, the whole of the other currents being utilised for electric light or otherwise.

ABSTRACTS AND EXTRACTS.

ABSTRACT OF REPORT FROM THE SUPERINTENDENT OF ELECTRIC TELEGRAPHS, QUEENSLAND, ON THE CONDITION OF HIS DEPARTMENT.

SIR,—I have the honour to submit for your information the following Report on the progress and condition of the Department under my supervision during the year 1874:—

EXTENSIONS.

The extensions completed since the date of my last Annual Report (13th June, 1874,) are as follows:—

1. A line from Roma *viâ* Mitchell Downs to Charleville, 169½ miles in length, was completed and opened for business on the 5th of October last year. The timber used in construction consists of cypress pine, with a few ironbark and gum poles of good quality, and it cost, exclusive of station-buildings, £7,118 3*s.* 2*d.*, or £42 per mile.

2. A line from Ravenswood to Millchester, 41½ miles in length, was completed on the 28th of November last year. This line is built of ironbark, and cost, including the station-building at Millchester, £2,849 2*s.* 2*d.*, or £68 13*s.* per mile.

3. A branch line from Roma *viâ* Surat to St. George, 125 miles in length, was completed and brought into operation on the 2nd of December last. It is built of cypress-pine, ironbark, and gum, and cost, exclusive of station buildings, £5,349 0*s.* 3*d.*, or £42 16*s.* per mile.

4. A line from Millchester to Charters Towers, 2½ miles in length, was completed on the 29th of December last. This line is built of ironbark, and cost, including the station-building at Charters Towers, £406 5*s.*

5. A line within the railway fences, from Brisbane to a point 2½ miles beyond Ipswich, 27 miles in length, was completed and placed in circuit on the 18th of January in the present year. This line is built entirely

of bloodwood and ironbark. It consists of six wires, five for the business of this department, and one for railway purposes. The entire section cost £2,566 8*s*., or £95 1*s*. per mile.

6. A branch line from Bundaberg to Burnett Heads, $11\frac{1}{4}$ miles in length, constructed for the convenience of shipping, was completed on the 26th of January last. This line is built of box and blue gum. It is worked by Siemens' alphabetical instruments, and cost, exclusive of station-building, £644 3*s*. 9*d*., or £57 3*s*. 3*d*. per mile.

7. A branch line from Waterview to the Lower Herbert, $25\frac{3}{4}$ miles in length, was completed on the 26th of January last. The wire for this line is stretched a distance of $16\frac{1}{4}$ miles on the Cardwell poles to Stone Creek, thence $9\frac{1}{2}$ miles of new line have been constructed to the township. It is built mostly of bloodwood and ironbark, and cost, exclusive of station building, £423 17*s*. 3*d*.

8. A branch line from the Pilot Station on Curtis Island to the Quarantine Station at Sea Hill, $5\frac{3}{4}$ miles in length, was completed on the 6th of February last. The wire for this line is stretched 2 miles on the Rockhampton poles, thence $3\frac{3}{4}$ miles of new line have been erected to the Quarantine Station. It is well built of the best hard wood obtainable in the locality, and cost, exclusive of station building, £194 5*s*.

9. A line from the Pilot Station on Curtis Island to Cape Capricorn, 26 miles in length, constructed for the convenience of shipping, was completed on the 10th of March this year; the wire is stretched for a distance of 5 miles on the Rockhampton poles, thence 21 miles of new line have been erected to the Cape. The timber used in construction consists of bloodwood, ironbark, box, and blue gum, and the section cost, exclusive of station building, £640 4*s*. 10*d*.

10. A second wire, 493 miles in length, has also been erected on the existing poles between Rockhampton and the Burdekin. When closely examined, extensive repairs and alterations were found necessary on this line, in order to make it sufficiently strong to carry the wire with safety. These repairs and alterations were thoroughly carried out by Government working parties, and the wire was stretched by contract.

It will be observed that the average cost of constructing these lines is somewhat greater than in former years; this is in some measure owing to the enhanced price of material, scarcity of suitable labour, dearness of provisions, and, I believe, in no small degree to the prosperous state of the colony.

Since the 13th of June, 1874, 25 stations were opened for business.

At the end of 1874 there were 3,616 miles of line, 4,891 miles of wire, and 88 stations in daily operation in this colony; and 201 officers, of various grades, regularly employed by the department.

We have now 3,678 miles of line, 4,975 miles of wire, 97 stations, and 210 officers on the permanent staff, together with 38 overseers, and men temporarily employed on construction and maintenance duty.

Meteorological and Shipping Reports.

Meteorological observations were taken at 9 A.M. and 3 P.M. daily throughout the year by officers in charge at Cape Moreton, Toowoomba, and Warwick, and the rainfall was duly registered at all stations every morning. The results, together with wind and weather reports, were transmitted to the principal offices and other stations requiring the information, free of charge. Free messages reporting the arrival and departure of shipping are still forwarded from the several coast stations, and to Sydney, Melbourne, and Adelaide, as regularly as the exigencies of the service will permit.

As this business is rapidly increasing, I would again draw attention to the inconvenience it causes, by taking up time on the busy lines that should properly be devoted to legitimate traffic. Shipowners and agents appear to be the principal people benefited by free shipping telegrams, whilst other members of the community, the majority in fact, take little or no interest in their transmission.

Under these circumstances I can but recommend that shipping telegrams should in future be placed on the same footing as ordinary business, and charged for at current rates. This system is generally adopted in Europe and America with perfect success. I believe that a similar arrangement would work well on the lines of this colony, and at the same time materially increase our slender revenue.

Working of Lines.

The several lines in Queensland have worked well during the year, the insulation continued satisfactory, and few interruptions occurred. However, communication north of Rockhampton was suspended for some days in February and March this year, during the prevalence of a cyclone. The line was seriously injured at Alligator Creek, near Yaamba, where the water rose sixty feet above its ordinary level, and submerged the

wires. Every effort was made to restore communication, which was at length accomplished by carrying wires fromtree to tree well above high-flood mark. In several places south of Rockhampton the lines were also injured by the same cyclone: the defects were, however, speedily repaired, and very little inconvenience was experienced by the interruptions, as other lines were available for the southern traffic. At the same time a break occurred at the Gilbert River crossing, which could not be repaired until the flood subsided.

From accidents, owing to various causes, the New South Wales lines between Sydney and Tenterfield have given more than usual trouble during the past summer, and Queensland business was at times delayed. However, inconvenience of this nature will hardly occur again, as I am informed that four wires are now available between Sydney and Murrurundi, three to Bendemere, and an additional line round by Bundana and Inverell to Glen Innes.

Wheatstone's automatic instruments, arranged for the duplex principle, have lately been introduced by the New South Wales department for the purpose of increasing the capacity of their intercolonial lines. By this method telegrams are transmitted by simply passing a band of perforated paper through the instrument, several clerks being required at each station to prepare and transcribe the messages.

The system is well spoken of in England, and, if economical to work, might be introduced by us with advantage.

An interruption occurred on the Keppel Bay line in August last, which continued for several days; the defect was caused by a boat's anchor injuring the cable between Curtis Island and the mainland. Steps were at once taken to make the necessary repairs, and communication was restored with the least possible delay.

The submarine lines in Moreton and Hervey's Bays have worked well throughout the year, and the insulation of all the cables continues perfect.

A portion of the line between the Gilbert and Einnesleigh rivers will shortly require considerable repairs; as white ants are numerous in this district, and timber is scarce, it will be cheaper in the end to import sufficient iron poles for the whole section, and erect them when necessary.

The number of paid and service messages transmitted during the past and previous years, together with the respective increase, is shown in the following table:—

	1873.	1874.	Increase.
Paid messages	124,464	225,975	101,511
Service messages . . .	31,804	85,044	53,240
International messages . .	340	395	55
Total . .	156,608	311,414	154,806

These figures disclose an increase of 101,566 in the paid messages, and 53,240 in those forwarded on Her Majesty's service, being a total increase of 154,806, or nearly double the number transmitted in 1873.

The Table contains a statement of amounts expended on construction account to 31st December, 1874. The total sum expended on telegraph works in this colony amounts to £223,239 0*s.* 9*d.*

It is gratifying to observe that the lines of Queensland, although so recently established, will bear favourable comparison, both in extent and efficiency, with the telegraph systems of many older and more densely populated countries.

International Communication.

The International Lines in connection with Australia have worked well during the year. The only important interruptions occurred on the Singapore-Madras and Singapore-Batavia sections, which were repaired in the course of a few days.

The Adelaide and Port Darwin overland line has also worked well. It has been seldom interrupted, and in every instance communication was promptly restored.

It is unsatisfactory to remark that the International business dealt with by Queensland stations is still inconsiderable; only 395 messages were transmitted and received during 1874, being an increase of 55 on the previous year. This may be accounted for by the high tariff still in force, no reduction having been made in the rates since the line was opened.

In October last year a meeting of representatives of Queensland, New South Wales, and New Zealand, took place in Sydney, for the purpose of making the necessary preliminary arrangements for constructing the proposed Queensland-Singapore and New South Wales-New Zealand cables, provided for by the respective Governments in 1873-4.

The meeting definitely decided that immediate action should be taken,

and special agents were appointed under the great seals of the three colonies, giving them authority to enter into a contract in England for constructing, maintaining, and working the cables.

Mr. Daintree, the Agent-General, was appointed to act for Queensland, Sir Daniel Cooper, Bart., for New South Wales, and Mr. Vogel to represent New Zealand.

By last advices from London negotiations were in progress.

During 1874 considerable advancement has been made in telegraph extensions throughout the world. Many important cables were successfully laid, the total mileage having by far exceeded that of any previous year.

I have, &c.,

W. J. CRACKNELL,

Superintendent of Electric Telegraphs.

The Honourable the Postmaster-General.

APPENDICES.

Electric Telegraph Department.

Return of miles of line, miles of wire, number of stations, number of officers, number of messages transmitted, and receipts and expenditure in each month during the year 1874:—

Number of miles of line, 3,616$\frac{3}{4}$.

Number of miles of wire, 4,891$\frac{1}{4}$.

Number of stations, 90.

Number of officers, not including construction or maintenance parties, 201.

Receipts:—

Number of paid messages, 225,975.

Amount of paid messages, £21,276 19*s.*

Number of messages O.H.M.S. 85,044.

Value of messages O.H.M.S. £7,039 19*s.* 10*d.*

Number of international messages, 395.

Proportion of international messages due to Queensland, £119 7*s.* 6*d.*

Total number of messages, 311,414.

Total amount, £28,436 6*s.* 4*d.*

Expenditure:—

Refundments to other Colonies, being proportion due to them for intercolonial business, £2,033 1s. 4*d.*

Total expenditure, £37,101 18*s.* 5*d.*

TABLE showing CONDITION of GOVERNMENT TELEGRAPHS in several of the European States and British India, during 1872.

—	No. of Miles of Line.	No. of Miles of Wire.	No. of Stations.	No. of Instruments in circuit.	No. of Officers employed.	No. of Messages transmitted.	Receipts in Dollars.	Expenditure in Dollars, exclusive of amount expended in construction of New Lines.	Population.	Average Number of Messages sent during the year in proportion to the Population.
Germany	17,661	60,816	3,058	3,253	5,569	10,158,041	2,272,239	2,226,272	34,378,253	·0660
Austria	11,995	31,510	1,672	1,285	2,750	4,796,127	1,429,735	1,397,594	20,394,498	·0701
Belgium	2,853	10,947	800	910	1,580	3,198,074	336,914	354,894	5,113,680	·0658
Denmark	1,529	3,988	169	207	408	603,317	129,387	116,355	1,784,741	·0725
Spain	7,287	16,572	215	408	1,826	1,304,260	354,883	697,876	16,732,052	·0218
France	29,481	79,690	3,463	4,147	5,260	8,052,403	2,531,747	2,539,000	36,102,921	·0701
Great Britain	23,878	99,918	5,505	7,542	10,576	17,407,103	5,088,330	4,568,445	31,628,338	·1608
British India	15,568	31,040	770	553	2,714	726,341	863,435	1,349,477	236,523,542	·0036
Portugal	1,929	3,550	120	187	650	319,280	69,847	162,303	3,829,618	·0182
Holland	2,039	6,992	282	345	914	2,031,089	257,961	375,186	3,637,279	·0709
Russia	34,914	68,858	1,475	1,565	5,481	3,259,552	3,424,042	2,582,589	78,394,471	·0436
Sweden	4,371	11,196	354	636	531	1,715,288	270,261	197,412	4,250,402	·0635
Switzerland	3,429	7,837	761	955	1,134	2,171,858	318,284	291,428	2,670,345	·1191
Italy	12,087	41,543	1,322	1,550	3,530	4,445,474	1,253,537	968,801	26,801,154	·0430
Norway	4,012	6,247	148	252	582	604,696	198,846	175,957	1,763,000	·1127

SEA TELEGRAPHY, IN SPECIAL RELATION TO THE LOSS OF THE VANGUARD.

A Paper read by Mr. W. H. Bailey before the Manchester Scientific and Mechanical Society, Friday Evening, 1st October, 1875.

To communicate by signal is a practice of considerable antiquity; but the time at our disposal will only permit of a very brief reference to some of the early means by which people separated by distance have been able to communicate with one another. The prophet Jeremiah exhorts the children of Benjamin to set up a sign of fire; and in profane history we have fires, beacons, and torches often alluded to. Agamemnon communicated with his queen by means of beacon fires on the mountain tops. Again, a very ingenious telegraph is described by Æneas, who lived in the time of Aristotle. It consisted of two tubs, marked inside to indicate the depth of the water, and furnished with small taps. The tubs were filled with water and placed on distant hills in charge of men with torches. Upon the elevation of a torch twice both taps were started simultaneously, and the water of course ran away. But the moment a torch was elevated by the sender of the message the taps were stopped. This system must have been adopted from the Clepsydra, or water-clock, which indicated the time of day by the height of water in a similar way. The ancient inhabitants of America would seem to have had communications of a similar nature. The use of torches and beacon fires by night would naturally lead to the use of the semaphore by day—including under this designation flags or wooden erections with wing pieces or circular plates, such as are in general use to-day on railways. The semaphore was brought into celebrity in France by three brothers named Chappe, who by means of wing pieces attached to the ends of a bar of wood were able to make a great number of combinations. The system soon became well known, and additions were made to it by subsequent inventors. Lord George Murray proposed his shutter telegraph in 1795. In the meantime, however, the navy had made but little advance, and ships could only speak each other by a clumsy system of flags by day and coloured lanterns by night. Rear-Admiral Sir Home Popham, in 1801, introduced several modifications in semaphores for land and sea, and also a system of spelling by means of flags. His ideas were adopted, and the celebrated words of Lord Nelson, "England expects that every

man this day will do his duty," were the first transmitted to the fleet by the new system, which was then on its trial as an experiment. Probably the greatest advance made in signalling at sea, however, has been the result of the joint labours of Captain Colomb and Major Bolton. These gentlemen have introduced a system of telegraphing at night by means of flash lights, using oil lanterns for short, and magnesium and other lights for great, distances. The inventions of these gentlemen have been so good that they have been adopted by the British and Foreign governments for communicating between vessels at night. They were used also on land in the Abyssinian Expedition. Captain Colomb and Major Bolton in their flash-light system have incorporated the idea of short and long lights; but it will be obvious that the invention, excellent as it is, will not meet all the conditions under which signalling is necessary, for the very best system of telegraphing at sea by signs may be rendered absolutely useless by a dense fog or smoke.

Hence it will be seen that telegraphs may broadly be divided into those which speak by sound and those which speak by sight. In the first we may include guns, trumpets, drums, gongs, and steam whistles, and in the latter flags, torches, lanterns, fires, semaphores, needles, and other instruments of a like character. We are chiefly concerned, however, with telegraphing by sound, of which I will now proceed to speak.

The experiments made by Dr. Tyndall in May 1873 demonstrated the possibility of hearing a sound of a definite volume at distances varying from $3\frac{1}{2}$ to $12\frac{3}{4}$ miles, according to the atmospheric conditions; but that on one occasion, when the wind was against the sound, it could be heard at a distance of $9\frac{1}{4}$ miles.

The instrument which I propose to use is a large steam whistle, so designed that a pianoforte touch will give a large opening to the steam-way, and enable a percussive short or long sound to be emitted. It will be observed that the centre spindle has upon it two equilibrium valves, which are so balanced as to be affected by the lever with the greatest ease, even though there may be a pressure of eighty or a hundred pounds to the square inch on the valve faces. This arrangement makes it possible for large whistles to be worked whose supply pipes may be three, four, five, or six inches in diameter. The bell of the whistle is adjustable in order that high or low pressure steam may be used, and the best results be thus obtained from the steam that the boiler may carry when it is at work. This steam whistle I propose to elevate about ten or

twelve feet above the head of the operator, who by means of a chain attached to the lever, and a handle, can work it and send messages by means of the dot and dash system, using either the Morse or any other code or alphabet. I prefer the Morse code for what appear to me sufficiently weighty reasons.

But, although I have so far called special attention to the utility of my system in foggy weather, it must have been apparent that it would be quite as useful under more favourable conditions. By this means, indeed, news could be communicated from one vessel to another as fast as a man could write, and with much greater facility than under the condition of things now prevailing. A vessel coming from a distant quarter of the globe meeting another, and having important intelligence to communicate, may do so by simply diminishing her speed, and no time need be wasted while boats are being sent with letters. What a great boon this would be to the newspaper press in general may be readily understood when it is borne in mind that the most that it is possible to obtain from outward-bound vessels are a few brief items of intelligence very hurriedly composed, whereas by this system all need for hurry is abrogated. As soon as the vessels got within hearing distance the communication could be commenced, and in the course of an hour very nearly a column of latest news could be transmitted—that is to say, supposing twenty words were sent per minute, 1,200 words would be sent in the hour, and that number is not far short of what appears in the Manchester papers morning by morning in the latest news columns.

EXTRACT FROM THE ANNUAL REPORT OF THE HONOURABLE WM. ORTON, PRESIDENT OF THE WESTERN UNION TELEGRAPH COMPANY.

To the Stockholders of the Western Union Telegraph Company.

The following Report of the operations of the Company for the fiscal year ended June 30, 1875, is submitted pursuant to the requirements of the bye-laws.

The gross receipts for the year from all sources, except proceeds of bonds, were $9,564,574.60; the gross expenses were $6,335,414.77; the difference, $3,229,159.83, being net profit. All sums paid as rental for leased lines are included in the gross expenses.

Compared with the preceding fiscal year, there was an increase in the gross receipts of $301,920.62; a decrease in the expenses of $420,319.06; and an increase in the net profit of $722,239.68.

There were in operation at the end of the year 72,833 miles of line, 179,294 miles of wire, and 6,565 offices.

The number of messages transmitted during the year was 17,153,710, being an increase of 824,454 over the preceding year.

In my last Annual Report reference was made to the possible necessity of putting down a new cable between Key West and Punta Rasa. That necessity became so pressing that in March last, at the request of the Directors of the International Ocean Telegraph Company, I proceeded to London, mainly for the purpose of contracting for the construction and shipment of a new cable for that line, superior in quality to any that had been previously provided.

A satisfactory contract for the manufacture of the cable was made with the India-rubber, Gutta-percha, and Telegraph Works Company of Silvertown, according to specifications prepared by Sir Samuel Canning, under whose supervision the work was carried on, and in June last the completed cable was put on board the International Ocean Telegraph Company's steamer "Professor Morse," which had been ordered to London for the purpose of receiving it. The steamer sailed direct for Key West, but during her voyage the yellow fever broke out at that and other Gulf ports, and, believing it unsafe to permit the vessel to go to Key West at that time, she was met on her arrival at the outer bar by an agent of the Company, with instructions to proceed to Port Royal, where she remained until a few days ago.

The Punta Rasa cable, which had been interrupted several times during the summer, involving a considerable loss of revenue and extra expenses for temporary repairs, gave out entirely a few weeks ago, since when communication has been maintained by steamer. While this is being written, information has been received that the "Morse" has successfully laid the new cable, which is working perfectly, and that telegraphic communication with Key West and Havana has been restored.

A fault has been discovered in the cable of 1869, between Key West and Havana, and, although the use of this cable is not required for the present volume of traffic, the new and better cable of 1873 being capable of passing double the present number of messages, the "Morse" will proceed at once to discover and repair the fault. When this has been done,

the other cable between Key West and Punta Rasa will be repaired, and, when this is done, the Company will then have two lines of cable from Punta Rasa to Havana.

Although the development of telegraphic business with Cuba has been greatly retarded by the condition of affairs on that island, the extension of the West India and Panama Company's cable system to a connection with the cables to Brazil, and the recent establishment of cables along the coast of Chili and Peru, is bringing a handsome increase of traffic to the International Ocean Telegraph Company's lines, and the prospects for the future are most encouraging.

General Review.

The growth of the Company's property and business during the nine years which have passed since the consolidation with the other principal telegraph lines is shown by the following statistics :—

From 1867 to 1875 the extent of line has increased from 46,270 to 72,833 miles, and the wires from 85,290 to 179,294 miles, being an increase of 57 per cent. of line and 110 per cent. of wire. The number of offices and stations has increased from 2,565 to 6,565—equal to 156 per cent. During the same time the number of messages transmitted has increased 192 per cent., the rate of tolls has decreased 51 per cent., and the gross receipts have increased 46 per cent. The average cost per message, during the same time, has been reduced from 67 to 37 cents, or about 45 per cent. The increase of 192 per cent. in the number of messages transmitted annually, while the mileage of wire has increased but 110 per cent, is explained by the fact that the number of messages transmitted per mile of wire has been increased 41 per cent.

The ability to make so large an increase in the carrying capacity of the wires is due in part to improvement in their conductivity and insulation, and in part to the introduction of the duplex and quadruplex apparatus, by means of which one wire is made to do the ordinary work of two, three, or four wires. By means of this apparatus, during the past year the Company has had the use of more than 30,000 miles of what may be called "phantom wire," which has cost nothing to provide, repair, and maintain, except the cost of the new apparatus, which is but little more expensive than that in general use, and is adapted to all the ordinary requirements.

The Atlantic Cables.

During the year covered by this Report, the Direct United States Cable Company has continued its efforts to establish telegraphic communication by an independent line between Great Britain and the United States, and althoughthese efforts were not successful until after the close of the fiscal year, the completion of the new line had been accomplished before the preparation of this Report commenced.

In May last the Anglo-American Telegraph Company, owning three cables between Valencia, Ireland, and Sidney, Nova Scotia, *viâ* Newfoundland, and one between Falmouth, England, *viâ* Brest, France, and the Island of St. Pierre and Duxbury, Mass., reduced the rate for transatlantic messages from four shillings to two shillings sterling per word. When the Direct United States Company's cable was opened for business in September last, the rate was fixed by both Companies at one shilling (twenty-five cents coin) per word.

During the period of about four months, while the two-shilling rate was in force, the number of cable messages increased about thirty-five per cent. over the number transmitted during the corresponding period of the preceding year. The shilling rate had been in operation less than a month when the new cable failed; thereupon the Anglo-American Company restored the four-shilling rate in force prior to the reduction which it had made in May last.

Although, by the terms of a contract entered into five years ago, the cables of the Anglo-American Company connect exclusively with the lines of the Western Union Company, the latter has no voice in deciding what the rates for translantic messages shall be. This Company receives a portion of the tolls on cable messages between New York and European stations, which tolls are fixed by the Anglo-American Company.

As the Western Union Company has been severely censured on account of the advance in cable rates, justice seems to require this statement of the facts. This Company has no more power to fix the rate for a cable message to Europe than we have to fix the rate for passage by steamer across the Atlantic. It is proper to add, however, that, on receipt of notice of the intention to advance the rate from one shilling to four shillings per word, an earnest request was sent to the manager of the Anglo Company in London to have their action reconsidered, and the rate fixed at two shillings a word. Our request was declined on the

ground that the revenue at the reduced rates had proved entirely inadequate to insure the permanent maintenance of the cables and the expense of operating them, and afford the proprietors any return upon their investment.

Improved Apparatus.

It has been known for years all over the world that signals could be transmitted through a wire much more rapidly by machinery than by hand, but the attempt to utilize this fact by the substitution of machinery in the transmission of messages for the key in the hand of the operator has never been successful, for the reason that the process is not economical either in respect to time or labour. A single message can be transmitted and copied in less time by the hand (Morse) process than by any other (except by the printing telegraph, which, for well understood reasons, is not adapted to general use). What the public requires is that each message shall have immediate despatch, and they have no other interest in knowing that, by waiting a while, the mere act of transmitting the signals which represent their messages can be performed by a beautiful process at an extraordinary rate of speed, except to know what to avoid.

The transmission and copying of a message at the other end of the circuit by the hand (Morse) process are simultaneous acts, both of which can be accomplished in as short time as a message can be prepared for transmission by the so-called automatic process, and both of which can be performed in less time than a message can be translated after it has been transmitted by the automatic process. So that, when the admirers of this mode have proved conclusively that the act of transmission by their process takes no time at all, they prove too much, because even then the time during which a message must inevitably remain in their hands is twice as long as is necessary with the other process. We have, therefore, declined to introduce processes, whatever their advantages otherwise might be, the substitution of which for those we now use would inevitably increase the time required for the delivery of a message at its destination.

The duplex and quadruplex apparatus, of which previous mention has been made herein, are fully realizing the expectations formed a year ago and set forth in my last annual report.

As the assertion has been frequently published that this Company does not control the patents for the quadruplex apparatus, it is proper to state here that the process known by that name was developed by Thomas A.

Edison, assisted by George B. Prescott, the electrician of the Company, while Edison was fulfilling an agreement made with me to perfect improvements upon the Stearn's duplex apparatus owned by this Company; and that subsequently a written contract for the sale of the quadruplex patents to this Company was executed by both inventors, and the sum of ten thousand dollars paid thereon.

It is true that, in face of these facts, one of the parties deliberately undertook to deprive the Company of that which he had sold to it, by giving subsequently a pretended title to another party; but the right of this Company to own and control exclusively that which it has purchased in good faith and partly paid for, will not, we think, be seriously disputed to the extent of a legal trial.

On account of the conflicting claims which have been set up, no patents covering the quadruplex apparatus have yet been issued in this country; but it is believed that such issue must take place during the coming season, and the control by this Company of the quadruplex method be thereby made complete. Careful attention is constantly being given to the subject of improvement in telegraphic modes and apparatus, and neither pains nor expense will be spared to secure for the Company the use of whatever will tend to promote the efficiency of the service.

(Signed) WM. ORTON, *President.*

NEW ZEALAND TELEGRAPH DEPARTMENT.

EXTRACTS from a REPORT of Mr. WILLIAM H. REYNOLDS, Acting Commissioner of Telegraphs, dated Wellington, 20 July, 1875.

The revenue for the year 1874–75 was estimated at £55,000, but it has exceeded that amount by over £800.

To enable masters of vessels to ascertain the state of the weather prevailing at any port to which they might be bound, or at any intermediate port, the system of sixpenny telegrams, including reply, was introduced. The facilities thus afforded, when generally known, will doubtless be taken great advantage of by maritime men.

During the past year 456 miles of new lines, carrying a single wire, have been erected, and 988 miles of wire have been added to the original lines, making a total addition of 1,444 miles of wire.

There are now opened to the public throughout the colony 127 stations, 21 of which have been opened during the past year, 6 being in the South Island, and 15 in the North Island.

The length of line maintained during the past year was 2,955 miles, the average cost for maintenance being £4. 16*s.* 4*d.* per mile.

At the close of the year 2,986 miles of line, carrying 6,626 miles of wire, were in circuit, showing an increased mileage upon the previous year, in line 456, and wire 1,444.

The nominal strength of the department, including linemen and inspectors, on the 30th June, 1875, was 509 against 388 of the previous year.

The duplex system of telegraphy, mentioned in the last Annual Report, has been in successful operation on the No. 3 wire in the Cook Strait cable since the 18th of June, 1874, and the advantage of speedy communication consequent thereupon has been very obvious. Instruments are now ready, and the system will be immediately introduced on the No. 3 wire north to Napier, and on the No. 3 wire between Blenheim and Christchurch. With the additional wires erected, and mentioned in another part of this Report, between Napier and Wellington, it is anticipated that this will greatly facilitate the transmission of the increasing work now offering.

It is proposed to introduce shortly the automatic system on some of the longer circuits, instruments for this purpose having just arrived from England. In the transmission of long press messages, which may possibly require to be sent in various directions, the saving of labour cannot be over-estimated.

Cook Strait Cable.

As will be seen from the insulation tests in Table H, this portion of the telegraph system in New Zealand still maintains its good working order. It has become a matter for consideration whether an alternate cable should not be laid, so as to avoid total suspension of telegraphic communication in the event of a breakage occurring to the present one—a contingency which, although remote, is nevertheless possible. It must be obvious that should such a contingency arise, and no alternative cable to fall back upon be at hand, very great loss and inconvenience to the public will ensue. The following memorandum by Mr. C. Lemon, the general manager of this department, upon this subject, points out the necessity for some action being taken in the direction indicated :—

" The present Cook Strait Cable has now been submerged close upon

eight years, and at the date of the last test for insulation (24th March, 1875) gave as good results as when first laid. This state of insulation may continue, so far as comparing previous tests taken monthly during the last six years is a guide; but the cable is liable to interruption from either of the two following causes: first, from a ship on a lee shore, off Cape Terawiti, or in that vicinity, endeavouring to save herself by letting go her anchors, and possibly fouling the cable with the same; second, by an earthquake causing a fissure in the bed of the ocean in a line at right angles to the lay of the cable, and thus causing it to part.

"Both these accidents have happened to cables; and, although the chances of the Cook Strait Cable receiving injury from either of the above sources is very remote, still they are accidents within the bounds of possibility.

"It is for the Commissioner to consider, in the event of interruption from either of the above sources, whether it would not be prudent to have a second cable laid (containing either one or three wires), as soon as it could be obtained from England, so that telegraphic communication, pending the repair, would not be entirely suspended, which it doubtless would be were an accident to happen to the present cable without an alternate one to fall back upon. I estimate the cost of a one-wire cable laid at £10,000.

"From a conversation I have had with Captain Fairchild, I was led by him to understand that there are other approaches on either side of Cook Strait equally as good for cable-landing purposes as the present.

"The present cable, owing to the introduction of duplex telegraphy, is capable of performing all the work which may be required of it for some time to come; but in the event of an interruption and pending repairs, and without a second cable to fall back upon, the pecuniary loss to the department would be great, whilst the public would be much inconvenienced by the total suspension of telegraphic communication with the other island.

"It is possible that, in the event of an interruption, the picking up of the two ends of the cable, and its repair, might occupy the best part of a month, should the weather prove unpropitious.

"The picking-up gear and steam-engine are always kept in readiness for such a contingency, and are under the charge of Mr. Nancarrow.—C. Lemon, General Manager."

(Signed) W. H. Reynolds, *Acting Commissioner of Telegraphs.*

TABLE F.

COMPARATIVE TABLE showing the Progress of the Telegraph Department during the Financial Years ended 30th June, 1866, 1867, 1868, 1869, 1870, 1871, 1872, 1873, 1874, and 1875.

Year ended	Number of Miles of Line.	Number of Stations opened.	Total Number of Telegrams forwarded during the Year.	Total Value of Business done during the Year.	Cost of Maintenance of Stations.	Cost of Maintenance of Lines.	Total Expenditure.	Cost of Maintenance of Lines per Mile.
				£ s. d.	£ s. d.	£ s. d.	£ s. d.	£ s. d.
30th June 1866	699	13	27,237	6,045 2 4	3,934 3 4	2,443 2 11	6,377 6 3	3 9 10
,, 1867	757	21	70,952	12,840 14 9	8,017 14 7	2,541 4 11	10,558 19 6	3 7 1
,, 1868	1,110	31	98,485	18,324 3 10	9,489 17 10	5,406 7 3	14,896 5 1	4 17 4
,, 1869	1,329	45	156,157	31,951 2 1	14,266 12 7	8,547 4 9	22,813 17 4	6 8 6
,, 1870	1,661	56	185,423	29,470 7 4	16,417 7 4	14,120 4 10	30,537 12 2	8 9 11
,, 1871	*1,976	72	312,874	32,296 6 2	21,254 4 3	11,344 3 8	32,598 7 11	5 19 6
,, 1872	†2,185	81	411,767	39,164 13 9	23,593 9 9	8,858 19 7	32,452 9 4	4 2 3
,, 1873	‡2,356	93	568,960	50,786 0 9	27,040 18 10	9,479 5 4	36,520 4 2	4 1 11
,, 1874	§2,530	105	752,899	59,127 10 4	38,801 19 4	15,021 17 11	53,823 17 3	6 3 11
,, 1875	‖2,986	127	917,128	68,981 3 0	45,814 11 4	14,240 19 7	60,055 10 11	4 16 4

* From this mileage 78 miles to be deducted before computing the cost per mile for maintenance.
† From this mileage 32 miles to be deducted before computing the cost per mile for maintenance.
‡ From this mileage 42 miles to be deducted before computing the cost per mile for maintenance.
§ From this mileage 106 miles to be deducted before computing the cost per mile for maintenance.
‖ From this mileage 31 miles to be deducted before computing the cost per mile for maintenance.

TABLE II.

INSULATION TESTS of the COOK STRAIT CABLE for the Years ended 30th June, 1868, 1869, 1870, 1871, 1872, 1873, 1874, and 1875 respectively, showing the Resistance per Knot after Two Minutes' Electrification in Millions of Ohms (British Association Units of Resistance).

Date.	No. 1 Wire.	No. 2 Wire.	No. 3 Wire.	Date.	No. 1 Wire.	No. 2 Wire.	No. 3 Wire
1867—				1871—			
September 1 ...	473	71	554	December 28 ...	570	9·98	1,079
September 30...	634	102	634	1872—			
October 14......	547	179	620	January 27......	579	8·29	981
November 1 ...	598	32·6	667	February 27 ...	596	9·5	1,073
November 14 ...	574	10·6	643	March 31	676	18·5	938
December 2 ...	608	10·5	709	April 25.........	553	39·1	979
1868—				May 28	649	44·4	1,173
June 7*	930	5·5	1,094	June 22	687	49·6	1,146
September 11...	630	22·5	946	July 23	826	65·6	1,476
September 17...	697	63·5	1,000	August 19	818	88·5	1,735
1869—				September † ...	...	...	...
March 27	467	26	729	October 22......	709	173	1,331
April 19.........	483	30	748	November 20 ...	763	208	1,556
May 11	562	31	950	December 30 ...	867	220	1,561
May 17	531	14	963	1873—			
June 22	522	15	931	January 24......	642	10·3	1,307
July 27	649	9	1,104	February 24 ...	578	18·1	1,271
August 24	667	7	1,200	March 25	611	24·8	1,420
September 27...	754	13	1,275	April 23.........	585	21·0	1,272
October†.........	...	...	...	May 23	505	29·7	1,231
November 8 ...	582	5	1,253	June 23	611	50·0	1,203
December 3 ...	638	2·5	1,159	July 24	628	61·3	1,159
1870—				August 23	717	68·3	1,564
January 22......	526	2	865	September 23 ...	609	57·2	1,193
February 22 ...	417	3	707	October 21	569	182·1	1,214
March 18	442	3·4	967	November 22 ...	695	347·5	1,429
March 20	344	3·06	619	December 24 ...	554	389·5	1,312
April 26.........	430	8·6	812	1874—			
May 31	456	·446	871	January 24......	592	314·3	1,438
June 26	355	2·3	622	February 24 ...	551	280·7	1,217
July 16	517	·393	893	March 25	559	371·5	1,106
August 16	473	1·7	763	April 24.........	579	356·4	1,303
September 17...	508	8·7	1,108	May 23	551	398·0	1,115
October 20......	471	5·5	1,069	June 27	607	96·1	1,227
November 21 ...	505	10·2	1,149	July 24	682	75	1,365
December 21 ...	480	7·7	1,121	August 29	771	530	1,564
1871—				September 24 ...	643	69	1,483
January 21......	574	6·7	1,312	October 26......	944	28	1,928
February 21 ...	565	2·0	1,173	November 24 ...	849	18	1,882
March 23	497	2·39	1,166	December 23 ...	650	84	1,060
April 24.........	508	1·85	1,003	1875—			
May 23	489	2·08	955	January 23......	883	94	1,695
June 24	717	1·28	1,288	February 23 ...	764	44	1,274
July†	...	...	...	March 24	719	30·4	1,277
August 25	751	1·36	1,444	April 23.........	636	37·6	1,139
September 25...	517	1·31	1,123	May 22	712	41·7	1,274
October†.........	...	...	...	June 24	562	41·9	1,064
November 23 ...	641	2·65	1,481				

* This test was taken with zinc to earth; all the others with copper to earth. † No test taken.

JOURNAL

OF THE

SOCIETY OF TELEGRAPH ENGINEERS.

VOL. IV. 1875. No. 12.

The Thirty-eighth Ordinary General Meeting was held on Wednesday, the 10th November, 1875, Mr. LATIMER CLARK, President, in the Chair.

The President rose and said—

GENTLEMEN,—In meeting together for the first time after the recess, I have much pleasure in stating that the business of the Society has been well attended to by the Council, and the preparation of the Catalogue of the Ronalds Library has been duly carried on, and is now in an advanced state. The work, however, is of such magnitude that I fancy it will be a long time before we can announce its completion; in the meantime it is going on, and about one-third of the catalogue has been written out and is in type.

The Paper for this evening is "On the respective merits and durability of Gutta-percha and India-rubber Joints," by Mr. Henry C. Mance, of the Persian Telegraph Department, a member of the Society, and well known to most of us, and I hope when that Paper comes before you it will be discussed in a way that the importance of the subject deserves. But, before we commence, it is my duty to take some notice of the loss of one of our most distinguished members; I allude to the death of Sir Charles Wheatstone. A greater name than that we can seldom expect to have in our Society. Sir Charles Wheatstone was distinguished, not only amongst the members of this Society, but throughout the world, as one of the most eminent men of science of this or any past

era, and I do not think we can duly appreciate the greatness of his name unless we regard it from a proper distance. You cannot discover the tallest tree in the forest while you are standing under its shadow. If you wish correctly to estimate the magnitude of a building, it is necessary to place yourself at a distance from it; it is only then you can fully realise its real proportions as compared with its fellows. So it is with the name of Sir Charles Wheatstone: I feel that, in order to appreciate how great a man he has been, we must look forward many years—I mean by that a very great many years—if we can take our stand in imagination, a thousand years hence the name of Wheatstone will still be well known and highly honoured. So far as we can judge from the history of the human race, and of the past, I am of opinion that, as long as history lasts, the name of Wheatstone will be associated with that of Watts and Stephenson as men who, in the era of Queen Victoria, were prominent in the introduction of those magnificent enterprises by which the whole world has been practically reduced to one-twentieth part of its former size. Our successors will hear in their day of the giants of the Victorian era; they will hear the name of Watts in connection with the steam-engine, and of Stephenson in connection with the locomotive and railways; and they will also hear of Wheatstone in connection with the electric telegraph. We who are closer to him, and know more of the history of the invention, are well aware that others are entitled to share with him in the fullest degree the honour of the introduction of the electric telegraph; but history is written very much by scientific men, and Sir Charles Wheatstone was himself an eminently scientific man, and mingled so much with scientific men, that those who will be the recorders of the history of the future will, to a great extent, associate his name alone with the practical introduction of the electric telegraph. I do not speak of the justice or injustice of the matter, but from the position he held here and on the continent it is certain, after we have passed away, that his name will be more prominently associated with the introduction of the electric telegraph than any other. Therefore, I say we lose in him an honoured name, and one of the most distinguished members our Society can ever expect to possess.

I think it will not be a loss of time if we, as an electrical society, consider for a few moments the great services which Sir Charles Wheatstone has rendered to the science of electricity; and, if it will not be detaining you too long, I will review briefly a few of the valuable improvements which he introduced in telegraphic and electrical science.

Our late lamented and distinguished member was born, as most of you know, at Gloucester, in the year 1802. His parents not being persons of affluence, he had to earn an honest living at the commencement of his life. I believe he was connected with the sale of musical instruments. He, however, soon rose above that. His talents became known, and in 1834 he was appointed Professor of Natural Philosophy at King's College, and a worthy choice they made, whoever chose him. He early distinguished himself in science, for in that same year, 1834, he made that marvellous experimental determination of the velocity of electricity which would have been of itself enough to render his name immortal amongst the roll of men of science. It must have been a happy inspiration which struck him—but, knowing the man, it ought not to surprise us—it was a magnificent idea of his, that method of using a revolving mirror to illustrate the velocity of electricity, and subsequently that of light itself—a velocity so great that by no other mundane means could we demonstrate its existence at all; but Wheatstone, in 1834, conceived the happy idea of causing a mirror to revolve at a velocity of 800 times per second, and by that means to demonstrate whether a current of electricity did or did not take an appreciable time to pass round a circuit of wire. In his lecture-room at King's College he suspended a length of about four miles of wire round the room, and he sent through those four miles of wire a discharge from a Leyden jar, and caused the discharge to form sparks and make itself manifest at three places —first at the instrument where it left the jar, second at two miles distance of wire, and lastly again when it re-entered the jar, and these three breaks were so arranged that they were all in a direct line opposite to him. He looked at these three points where the sparks passed in a mirror revolving at an immense velocity. It was nothing more than the works of a watch, on which he fixed his

tiny mirror, and at the time when the current was passing in the right direction he saw the reflection of the three discharges from the Leyden jar. Now, if these discharges were all absolutely simultaneous, the mirror would show all the three in the straight line in which they were arranged; but if not simultaneous, the middle one being a little later, the mirror would have turned through a minute portion of space, and its reflection would not be in a straight line, but reflected at a point a little distance from the others, and the three sparks would not be in a straight line but in a slightly curved line. This magnificent experiment Sir Charles Wheatstone carried out successfully in his own lecture-room, to his very great delight, as we may readily imagine. He saw that these sparks were not in a straight line, and that the current had taken some time to traverse the three points of the wire and reach the end of the circuit. From this it was deduced that the velocity of electricity is 288,000 miles per second, which does not vary very materially from the velocity of light, and Wheatstone announced this as the result of his wonderful experiment, and it at once spread his name through the world of science, as the author of one of the most magnificent discoveries ever recorded. Since then that system has been used for determining the velocity of light, and the revolving mirror is now one of the most powerful means of determining high velocities and minute portions of time. In determining the velocity of electricity we know, as Faraday pointed out, that there is no such thing as a constant velocity of electricity; in fact, no one knows what the ultimate velocity of electricity is; we know however what takes place in such an experiment as this; there is the same phenomenon in a submarine cable—that is, before the spark could cross the middle space at two miles from the battery, it had first to charge the two miles which existed between the battery and the conducting knobs to a tension sufficient to strike across that distance, and the time it took in charging that two miles of wire would depend upon the conductivity of the wire, and also on the amount of its electrostatic capacity. Faraday showed that if you put Leyden jars in circuit, or if you have a submarine circuit instead of the walls of a room, the velocity, instead of being 288,000 miles per second, might only

be 144,000 or less, the function of velocity evidently depending upon the conductibility and the amount of electrostatic capacity of the wire; therefore, though Wheatstone supposed that he had discovered the determinate velocity of electricity—we know that it is in some cases more and in some less—that it is in fact entirely dependent upon the two functions I have stated. Still, the experiment was one of so brilliant a character that alone it was enough to immortalise his name.

In 1835 he made another discovery, and ascertained that when metals were *volatilised* by the electric spark—that is, when a current was passed between two terminals of different metals—the spectrum when viewed through the prism showed certain determinate lines of light, and he showed also that these lines of light varied in different metals, and he went so far as to say that it was possible to determine what kind of metals the terminals were composed of by looking through the prism and observing the position of the lines of light which were shown on the spectrum.

He in fact distinctly foreshadowed the method of spectrum analysis which has since made us acquainted with the nature and composition of the fixed stars.

In the same year (1835) Wheatstone was engaged at King's College in giving lectures on electricity and other subjects of natural philosophy. Amongst others, he gave a lecture to his class on Baron Schelling's telegraph, which was, to a great extent, the same telegraph which was afterwards so largely introduced in England. In doing so, he pointed out that there must be a great future before such a force as we possessed, and he thoroughly demonstrated to the world that there was the possibility of such an invention as the electric telegraph, and that the means were then known by which it could be introduced. This was his first association with the electric telegraph.

In the following year we come to a period of great importance in Wheatstone's life. It appears that Sir William—then Mr.—Cooke, on the 6th of March, 1836, saw some experiments which were made at Heidelberg with Professor Muncke's telegraph, and he was so struck with the important nature of these experiment that he abandoned the occupation he was then following—that of a

surgeon,—and came to England with the determination to pursue the question of the electric telegraph to its fullest end. He accordingly arrived in England on the 22nd of April, 1836; but it was not till January, 1837, that he had so far advanced the question he was dealing with as to be able to exhibit it publicly in London. He was not himself a man of great scientific knowledge. He was, as I have said, a surgeon, and, feeling his want of scientific knowledge, he twice consulted Professor Faraday and Dr. Roget, as the best persons he could go to, with the view of carrying out his pet idea of bringing the electric telegraph into practical use on the railways, especially in tunnels, where there was great necessity for it. By the advice of Dr. Roget, on the 27th of February, 1837, Mr. Cooke visited Wheatstone at his house in Conduit Street, who afterwards introduced him to his lecture-room at King's College. He then proposed to Wheatstone that they should enter into a partnership to carry out the electric telegraph, and, strange to say, Wheatstone was at first disinclined to enter into an arrangement of the kind. We all know how very frequently a small circumstance may determine the future course of a man's life. He may be late at dinner, he may pass on the right hand side of a street instead of the left, he may be civil or inattentive to a man, and a trivial circumstance may produce an entire change in his future career. Wheatstone, in my opinion, will owe much of that celebrity which I think he will obtain in future years to his connection with the electric telegraph, and I believe had he not associated himself with Sir William Cooke he might have remained only a distinguished professor who had demonstrated and explained the possibility of electric telegraphy. But fortunately he took the other course, and did enter into partnership with Cooke. Reluctant at first, he might have contented himself with saying, "I have made a great name in science, whereas you are not a scientific man, and my intention is to continue my experiments and give the world the benefit of them." Cooke, however, took another view of the matter. "I want," said he, "to make money by the telegraph, and I want your assistance; let us work together and treat the thing from a commercial point of view instead of a merely scientific one." It was proposed that a patent should be

taken out, and in those days a patent cost several hundred pounds; and Wheatstone consented to become partner in the concern. I look upon that as the turning-point in the greatness of Wheatstone; for I suppose, had he not taken that resolution, he would have remained simply as one scientific man amongst a great many others of the present era; but having joined Cooke, and having with him successfully introduced the electric telegraph, he has gained for himself a higher pinnacle of scientific eminence than would have been possible by any other career. It was in May, 1836, that Cooke and Wheatstone resolved to unite together in this great scientific enterprise, their deed of partnership being dated the 19th of November, 1837. On the 25th of July, 1836, the trial of the telegraph was made from Euston Square to Camden Town. That may be regarded as the first piece of practical electric telegraph which has existed, and was the origin of the great telegraph system which has since spread all over the world. Wheatstone was able to foresee, at this early period, what we all now so well know, the value of Ohm's laws. He was the first to appreciate and employ Ohm's formulæ long before others recognised their importance or their truth. I may add that in the same year, 1836, Wheatstone was elected worthily a Fellow of the Royal Society.

In 1837 Cooke and Wheatstone took out a patent for the five-needle telegraph, which is essentially the child of Sir Charles Wheatstone. This consisted of a keyboard of peculiar arrangement, two opposite electro-magnets deflecting a needle. The instrument, however, never came into practical use, but, like all he did, it was eminently ingenious. The opposed electro-magnets were, however, a very important introduction in telegraphy. They also patented—I do not know which of the two introduced it—but I believe it was Sir Charles Wheatstone who invented—the system of sounding distant alarums by removing a detent of clockwork by the aid of a local battery, contact being made in mercury by a needle dipping in cups. All these important introductions were patented in 1837. They proceeded further with their inventions, and in 1839 the telegraph line was laid from Paddington to Drayton, and extended to Slough in 1841, and rendered such service in leading

to the arrest of the Quaker murderer, Tawell, that it at once brought the electric telegraph into popular favour.

In 1848 Sir Charles Wheatstone suggested a very important step in the march of the electric telegraph. Before the Select Committee of the House of Commons on Railways he gave his opinion in evidence that it was quite practicable to construct a submarine telegraph from England to France, from Dover to Calais, or from Dover to Boulogne. As far as I know, he was the first to say he believed in the practicability of submarine telegraphy. That is now thirty-five years ago, and few at the time could have expected to witness what has happened since then. At this time (1840) Sir Charles introduced another invention, namely, the chronoscope, for measuring small intervals of time by means of electricity, more especially the velocity of projectiles, and I need not say what an important part that is now playing in the science of warfare. In the same year, also, he took out a patent for a form of alphabetical telegraph, showing letters by an escapement and rotating commutator, a step-by-step motion. In the same year Cooke and Wheatstone erected the telegraph line on the Blackwall Railway, the first practical line, after the line to Slough, on the Great Western Railway.

In 1841 they patented a type-printing electric machine, which was then a great novelty. It was a machine with a step-by-step motion, similar in principle to the one previously introduced in 1840, showing letters on the dial, but it also carried round the type on a number of light springs. This machine was worked by two wires, and, when the type was brought round, a hammer, acted on by a second current, fell upon it, and printed it on the paper. That was the first type-printer, a machine of which we have since seen such great developments. He also used a rotating magnetic machine, in which five coils revolve between six permanent magnets, with a commutator to give permanent currents, and he introduced an improvement of similar character in the electro-magnetic engine. He likewise introduced the well-known rheostat or resistance coil, which is in such constant use at this day, his first machine being a perfect resistance coil of a most elegant character. He also invented a chronoscopic method of recording time by a surface carrying

paper ruled to represent equal intervals of time, revolving uniformly by clockwork, and an electro-magnetic armature carrying a pencil. By this means the exact moment of the passage of a star or other phenomenon could be recorded, and this instrument is now fast coming into use in all the large observatories of the world.

I may here mention an incident in connection with the Blackwall telegraph which is interesting, and, I believe, true; I will mention it as stated to me by Mr. Greener, one of our members, twenty years ago. In 1841, probably in September or March, a very high tide occurred, which caused the inundation of the Blackwall Railway, and it rose to such a height that it injured the piping through which the wires were carried, and reduced the number of working wires from seven or eight—they were then using a wire to each station—to one or two. Mr. Cooke, who was the practical engineer of the telegraph, was in great trouble, fearing that some accident might ensue by the failure of his telegraph, and by their being unable to communicate with the intermediate stations from the Blackwall end of the line. I have been assured, not only by Mr. Greener, but also by another telegraphic clerk on the Railway, that they had previously arranged a code of signals on one wire by deflecting the needles alternately, once, twice, or thrice, to the right or left, as we do now with the single-needle telegraph, and had managed to carry on communications respecting their dinners and other private matters. Mr. Cooke, on being informed that it was still possible to telegraph, gladly availed himself of the new means of communication by one wire, and from that moment our well-known single and double needle instrument was practically invented. If these statements be accurate, the first idea of the double-needle telegraph did not originate either with Wheatstone or Cooke, but was suggested by Mr. Greener and his partner, who was at the time engaged with him on the Blackwall telegraph.

In 1841 a difference arose between Wheatstone and Cooke as to who was the real inventor of the telegraph, and I think we must all now admit that neither of them was solely. They however agreed to submit the question to arbitration—Isambard Brunel acting on the part of Mr. Cooke and Professor Daniell on that of Mr. Wheat-

stone. I will just read to you an extract from their award, and it is a very short one. They say: "Whilst Mr. Cooke is entitled to stand alone as the gentleman to whom this country is indebted for having practically introduced and carried out the Electric Telegraph as a useful undertaking, promising to be a work of national importance, and Professor Wheatstone is acknowledged as the scientific man whose profound and successful researches had already prepared the public to receive it as a project capable of practical application, it is to the united labours of two gentlemen so well qualified for mutual assistance that we must attribute the rapid progress which this important invention has made during the five years since they have been associated.—M. I. BRUNEL, J. F. DANIELL."

I am not aware that anything important in the history of Wheatstone occurred in 1842; but in 1843 he read a paper before the Royal Society, which was called "An Account of several new Processes for determining the Constants of a Voltaic Circuit;" and that paper I have always thought one of the most valuable and instructive papers that I am acquainted with. There is no member of this Society at the present day, however high his station or great his knowledge, who can read that paper through again without interest, and those who do not know it ought to make themselves acquainted with it. Considering the early date at which it was written, a more able paper on electricity I have never read. In the first place, he introduced to the notice of the world the principle of the Wheatstone bridge, now so familiar to all. We are aware it was not his invention, nor did he ever claim it. It was discovered and invented in 1833 by Mr. S. W. Christie, who published a description of it in the Philosophical Transactions; but the form in which he put it was not that of the parallelogram with which we are all so familiar; one side of the parallelogram was doubled back across the other, which gave it a confused appearance, and perhaps from this cause it did not attract notice; but Wheatstone, by embodying it in his paper in 1843, at once brought it into favour, and from that hour to this the instrument has been in the hands of every electrician. Wheatstone always used Daniell's battery in his experiments and lectures, and explained its advantages in this paper. In the same communication he describes the system

of units of electrical resistance. His unit was one foot of copper weighing 100 grains. He showed how this might be applied to the measurement of *distances* as well as electrical resistances, and practically he was the father of the system of measurement of telegraphic resistance; the paper as a whole, abounding as it does in simple but practical formulæ, for the calculation of resistances and of electric currents, is one of which it is difficult to speak too highly, considering the early date at which it was written. In 1843 he invented a machine by which, by making contact with the mercury in the bulb of a thermometer, he was able to register the observations of meteorological instruments at great distances, and to record the meteorological or physical changes going on. Such an instrument is one of considerable importance, and has been practically in use ever since.

In May of the same year Cooke and Wheatstone introduced a system of giving audible signals by striking a bell, using the derived current with a sensitive signal apparatus. This had been done by others, but theirs was a decided improvement of the telegraph, and brought into notice the system we now know as the sound telegraph. Also in the same year Wheatstone discussed the laws of derived circuits, and it is needless to say how valuable these are to us as electricians. He used shunts, and patented the method of applying stops to the needle and giving signals by successive right-and-left movements, although this had apparently been done two years previously on the telegraph of the Blackwall Railway. He also introduced in the same year a method of covering electrical conducting wires with leaden tubes, which to some extent foreshadowed the custom of the present day.

At this period of time Cooke had so far worked out the business part of the undertaking that on the 2nd September, 1845, the Electric Telegraph Company was registered, and commenced its operations in 1846. Wheatstone, by his arrangement with his partner, received 33,000*l.*, a substantial reward for the eminent services he had rendered to telegraphy.

From 1845 to the year 1858, so far as I remember and know, Wheatstone appears to have dropped entirely out of the telegraph connection, and I often wondered why a man who had made

so great a name did not again come to the front and bring his great electrical knowledge and inventive faculties into use; he did not do so, but, having received his well-deserved reward, he appears to have retired entirely from the field. After the lapse of some thirteen years, however, he introduced his system of automatic printing, that beautiful apparatus for transmitting messages at great speed by the punching of holes in paper. The idea was originally due to Bain, who in 1846 described and tried to introduce the system even in that early period of telegraphs; but it did not come into use, and, though many similar attempts were made by myself and others, all failed from our not having the requisite mechanical skill to make it practically successful. Wheatstone, however, aided by that admirable mechanician Mr. Stroh, brought out the beautiful instrument with which we are now so familiar and which is in hourly use, and he alone deserves the merit and credit of having made automatic telegraphy a perfect success.

In the same year, or perhaps a little later, he introduced that beautiful little instrument with which we are equally familiar, viz. the alphabetical dial telegraph, worked by the hand, and now in universal use; and in connection with that I need not point out where one of the secrets of Wheatstone's great success lay. He perceived distinctly what others did not, that, having to deal with extremely delicate forces and feeble currents of electricity, it was hopeless to attempt to employ heavy machinery, but that it was necessary to use the smallest and lightest appliances. In making his first experiments he used needles six or eight inches long, weighing half an ounce; subsequently the working parts were reduced to a small size, combined with exquisite workmanship, and weighing only a few grains. He made all his apparatus of a very light and highly delicate character; and it is the lightness and delicacy of the alphabetical and many other of his instruments which constitutes one great secret of their perfection and success. Wheatstone made, subsequently to 1858-9, some other very important inventions. Amongst these was a method of driving magneto-electric clocks by a series of revolving magnets worked by pulsating currents. He likewise

introduced an improvement in over-house telegraphs, and was the originator of the plan of suspending wires most in use in the present day. Latterly he experimented on a very delicate system of electrical communication, that is, with a drop of mercury in a capillary tube with a column of acid on each side; and he proved the fact that the most delicate current passing through a capillary tube of acid will cause a globule of mercury to move to the right or left, according as the current is directed through it.

I have now completed, so far as I know, the list of the principal discoveries of Sir Charles Wheatstone in electrical science. It now only remains to point out what great results have followed from the introduction of this system. I am informed that at the time of the transfer of the electric telegraphs to the General Post Office, in January 1870, the number of automatic instruments sending messages on punched paper was only six. There are now 140 in constant use. At the time of the transfer there were 644 needle instruments in use throughout all England. At the present day the number is increased to 3,941. At the same period there were 39 dial alphabet instruments in operation. There are now 4,178. I am further informed that at the present time by day there are 14,030 miles of wire worked automatically; by night 19,000 miles are hourly and momentarily in use on that system. I have received the further information that the railway mileage was, in 1870, 45,000 miles of wire, of which 35,000 were worked on the needle system of Wheatstone. We see what results have followed, and how far at this day it is appreciated and in use, when we learn that the present Post Office mileage amounts to no less than 108,000 miles at work in the telegraph service of this country.

I think I have mentioned most of the important contributions of Sir Charles Wheatstone to electrical science, but, if there are any which I have omitted, I hope some member will describe them. I may add that he was to the day of his death incessantly working upon new ideas, and I have no doubt there exist many unpublished experiments in which he was engaged, and the knowledge of which he has left behind him; I have only to express the hope that those who have access to his papers will kindly at some day give them, if not to this Society, to the

world at large, believing as I do that they will be of considerable interest and importance.

Dr. C. W. SIEMENS (past President) said: Mr. President and gentlemen, I have a proposition to make which I am sure you will all second and approve, that is, to ask our President to allow the address which he has just given us to be printed—not only in our Proceedings, but separately, and circulated to the members. I have listened, and I have no doubt all present have listened, with great interest and pleasure to his address regarding the works of our most highly respected and esteemed member, the late Sir Charles Wheatstone. He has done it, I consider, in a very masterly manner. I have read other notices regarding the works of Sir Charles Wheatstone, amongst others the address of M. Tresca before the French Academy, and I have taken part in a memorial addressed by the Royal Institution in his honour, but I have not hitherto found his works recorded in such a temperate, just, and complete manner as has been done this evening by Mr. Clark. We should honour the dead, but we should also be just with regard to them, and, whilst we avoid fulsome praise, we should take care to give them fairly and fully that amount of credit which is due to them. In the case of Sir Charles Wheatstone that amount of credit is a very large amount indeed. Sir Charles Wheatstone laboured during a period of between thirty and forty years incessantly in the field of science, and his fertile mind has produced results such as few have been allowed to attain; therefore he can well afford to have his works justly dealt with, and they need not be increased or diminished by one iota. There are one or two points mentioned by the President which I think could hardly be claimed for Sir Charles Wheatstone. I would mention the one regarding the effect of electricity in the capillary tubes. Sir Charles Wheatstone followed up the experiments on that subject with his usual energy, but I believe the idea was first suggested at Frankfort by Professor Lippmann. I think our President will be too glad to correct any excess of credit; it would not be a credit to Wheatstone, but rather detract from his real merits, if anything that was not fairly due to him was attributed to him. I think however, on the whole, we have heard an address regarding the

works of Sir Charles Wheatstone which deserves to live amongst us as a lasting record of the works of one of the greatest men this century will have to boast of. I beg to propose that our President be requested to allow his Address to be printed and circulated amongst the members.

Professor Abel: I beg to second the motion which has been made by Dr. Siemens. In doing so, I feel I represent here a gentleman who would have been more worthy, in every respect, to second it, namely, Mr. Sabine, the son-in-law of Sir Charles Wheatstone, who, owing to unavoidable circumstances, is not present this evening. But I believe I may claim—though my claim is a very modest one—to say a few words with reference to the great labours which Sir Charles Wheatstone has carried out, and the great success of his works. I feel happy in the recollection of the fact that nearly a quarter of a century ago I was associated with him in a humble way in one small branch of the many subjects which he took up with such enthusiasm, and I may be allowed to say there can be no man existing in the Society of Telegraph Engineers who ever was associated with Wheatstone who will not feel proud of that association, however humble it might have been. It was my fortune more than twenty years ago to work with Wheatstone in the application of high tension electricity to the explosion of mines, and I consider the enthusiastic way in which he entered into this subject led to the rapid development of that very important application of electricity which we have had on many occasions opportunities of noticing, and from which I believe this country will reap many advantages in the future. It was Wheatstone who proposed the application of magneto-electric currents to the explosion of mines, and he was the first to bring forward a powerful magneto-electric machine, which was first experimented upon by Mr. Henley for this purpose. He brought under the notice of the Government the successful labours of Du Moncel, Savari, von Ebner, and others in this direction, and I consider if he had only done this service he had done an important work. He did more, he constructed the first practical and thoroughly efficient magneto-electric machine for the explosion of mines, and it was at his

instigation that I devoted time to devise the actual means of doing this by his machine. With him, to undertake a work was to set about it with remarkable enthusiasm, to which was added an astonishing amount of ingenuity, and we cannot fail to notice how very rapidly he brought his scientific ideas into practical use. I consider we have had brought before us a most lucid and in every way a most valuable summary of the life of Sir Charles Wheatstone as presented by his works, and therefore I beg most heartily to second the motion of Dr. Siemens.

The proposition having been carried by acclamation,

The President: I have to say how deeply I feel the loss we have sustained in the death of our respected member. I think it would be proper, in a case of this kind, that we should pass a vote of sympathy on the part of this Society to the relatives of Sir Charles Wheatstone. I need scarcely tell you, what you would almost surmise, namely, that we had warmly hoped to have been honoured by nominating him as our next President, and it had been decided to communicate with him for that purpose when his lamented death intervened. In fact, the wish had been already communicated to his friends. I therefore propose that we offer a vote of deep condolence to the relatives of Sir Charles Wheatstone on the part of this Society.

Major Malcolm: I am quite sure that not a single hand will be raised in controversion of the proposition which has just been made by the President. I think we all cannot but feel that this Society has experienced a most serious loss in the death of Sir Charles Wheatstone, and I think, feeling it ourselves, we must feel with his relatives for the great loss which they themselves have experienced. It was not alone to telegraphic and electrical science that Sir Charles Wheatstone devoted himself, but, as we know, he applied himself to the whole range of science. I have the greatest, I can hardly call it pleasure, in seconding the President's proposition, and I trust you will allow me to put it to the meeting.

The proposition was then put and unanimously agreed to.

ON THE RESPECTIVE MERITS AND DURABILITY OF GUTTA-PERCHA AND INDIA-RUBBER JOINTS.

By Henry C. Mance,

Government Telegraph Department, Persian Gulf.

The following experiments were conducted at the head quarters of the Persian Gulf Telegraph department, Kurrachee, with the view of ascertaining the extent to which the different descriptions of joints might be relied on. The results obtained are communicated by the desire of the Director-in-Chief, in the hopes of eliciting further information on the subject from the Members of the Society of Telegraph Engineers. (See pp. 338, 339.)

Between fifty and sixty joints were submitted to observation. The jointer, who has had several years' experience, was aware that they would be carefully examined, and I have every reason to believe that great care was taken in preparing them.

It having been suggested that gutta-percha joints are more liable to perish when exposed to the influence of water in the cable tanks, joints 8 to 17 were submerged in the Kurrachee Harbour for six months; the deterioration however was about the same as that previously noticed in those kept carefully wetted in the tanks.

The insulation of the Fao-Bushire gutta-percha cable, which has scarcely been touched since it was first laid, is higher now than it has ever been, leading to the inference that gutta-percha joints made in the factory at home, when the core was new, are not subject to such a decrease in their insulation resistance as is shown in those recently examined at Kurrachee. The question arises whether gutta-percha a few years old can be jointed so successfully as that in newly-manufactured core. I do not speak of core the outside coating of which is perished, but sound core, which, although well preserved, has been manufactured say five or six years. Should a joint properly made in good core of this age

retain for twelve months the high resistance it shows when first tested?

Mechanically the *whole* of the joints were good and sound; there was no outward sign whatever to indicate any deterioration, and when cut open the workmanship appeared perfect. This remark applies also to joint No. 3, gutta-percha india-rubber. With the exception of No. 3 the india-rubber gutta-percha joints are, I think, very satisfactory. No. 4 is equal to one made altogether of india-rubber.

The No. 2 india-rubber joint is the only one of that kind which tests low, but this is due not to bad jointing but to a small perforation in the core made by a "borer" whilst the joint was submerged in the Kurrachee Harbour. It is worthy of note that out of a large bundle of joints only one india-rubber joint was touched by the borer, and in this case it had not quite penetrated through to the conductor. Many of the gutta-percha joints in the same bundle were riddled with holes by borers in the same time.

Thinking it possible that the gutta-percha joints 7 to 17 might have stood the test of time better with less manipulation during manufacture, I had joints 18 to 23 made with greater dispatch. The latter joints tested the best. I submitted also some sealed ends of percha core to examination and obtained very satisfactory results; it is very probable that joints are as often injured by over care as by too hasty manufacture. There is a point beyond which manipulation and the use of the spirit lamp will do positive harm.

Most of these joints were tested on several occasions, and the results obtained were fairly regular, usually showing a gradual decrease up to the last test, except in the case of the india-rubber joints, which remained steady. Every precaution was taken to prevent surface leakage over the ends of the joints; the testing-room was well warmed to counteract the moisture in the atmosphere outside, and the ends were carefully dried a few moments before testing.

The sensibility of the galvanometer used was such that the testing-battery of 80 cells would produce a deflection equal to 200,000° through a million units, consequently when a less deflec-

tion than 2° was obtained through any joint the resistance of the latter must have been at least 100,000 millions.

The good joints were also tested by Clark's Accumulation Test, using a trough suspended between two ebonite bars. The accumulation observed from a joint of which the resistance was known enabled me to roughly estimate the insulation of other joints, the resistance of which was too high to be measured by ordinary methods. I have considered it sufficient to record the good joints as over 100,000 millions, although I have no doubt many were more than double this. I did not observe the time required for the fall of tension to one-half, as unless the weather happened to be extremely favourable there was a great difficulty in preventing a slight leakage of one or two degrees on the connections. These observations have already extended over a period of more than two years; several joints were cut open, but the whole of those included in the list have, with one exception, been replaced in the tanks for further examination.

The President: I am sure I need not bespeak attention to this subject, as the importance of making good joints is obvious to us all. We have Mr. Willoughby Smith with us to-night, than whom no one knows more about joints, and I hope he will favour us with a little of his knowledge and experience, and at the same time tell us whether he has found any difficulty in the union of old with new gutta-percha, and whether, in joining old gutta-percha, it is necessary to use thin sheets of old gutta-percha, or whether he would employ new material for the purpose.

Mr. Willoughby Smith: The question has been asked, whether it is better to keep joints in water or out of water. My experience goes in favour of keeping them in water. In 1866 a cable was manufactured to be laid in the Behring Sea. The core was supplied by the Gutta Percha Company, and the cable manufactured by Mr. Henley. The cable left England coiled dry in the hold of a sailing vessel. After a long absence, it was returned to Mr.

TESTS OF EXPERIMENTAL JOINTS AT MANORA.

Description of Joint.	Distinguishing mark.	Date of manufacture.	Original resistance in millions of units.	Resistance when tested 15th January, 1875.	Resistance when tested 29th Sept. 1875.	Remarks.
India-rubber and gutta-percha	No. 1	May 20th, 1873	Over 100,000 millions	186 millions	720 millions	
do.	2	June 25th, 1873	„ 100,000 do.	56 do.	30 do.	
do.	3	June 21st, 1873	„ 100,000 do.	5 do.	1 do.	
do.	4	do.	„ 100,000 do.	Over 100,000 do.	Over 100,000 do.	The only joint of this kind which has not deteriorated
do.	5	May 29th, 1874	9,100 do.	170 do.	160 do.	
do.	6	do.	60,000 do.	1,700 do.	500 do.	
do.	7	do.	45,000 do.	56 do.	30 do.	
do.	8	do.	Over 100,000 do.	186 do.	80 do.	
do.	9	do.	393 do.	63 do.	50 do.	
do.	10	do.	27,000 do.	186 do.	77 do.	
Gutta-percha to gutta-percha	No. 1	June 25th, 1873	80,000 do.	6,600 do.	1,000 do.	
do.	2	do.	100,000 do.	746 do.	540 do.	
do.	3	Mar. 24th, 1874		1,600 do.	400 do.	
do.	4	do.		400 do.	300 do.	
do.	5	Mar. 31st, 1874		140 do.	81 do.	Buried two feet deep in sand for six months
do. (2 lapping only)	6	do.		224 do.	81 do.	
do. (2 do.	7	May 20th, 1874	13,000 do.	311 do.	76 do.	
do.	8	May 29th, 1874	60,000 do.	28 do.	11 do.	
do.	9	do.	550 do.	28 do.	11 do.	
do.	10	do.	60,000 do.	41 do.	15 do.	
do.	11	do.	60,000 do.	80 do.	40 do.	
do.	12	do.	60,000 do.	51 do.	30 do.	
do.	13	do.	13,000 do.	116 do.	55 do.	
do.	14	June 11th, 1874	Over 100,000 do.	60 do.	40 do.	
do.	15	do.	„ 100,000 do.	112 do.	65 do.	
do.	16	do.	„ 100,000 do.	225 do.	105 do.	
do.	17	do.	3,400 do.	80 do.	35 do.	
Three fathoms of gutta-percha core		1863	Over 100,000 do.	Over 100,000 do.	Over 100,000 do.	

Description of Joint.	Distinguishing mark.	Date of manufacture.	Original resistance in millions of units.	Resistance when tested 15th January, 1875.	Resistance when tested 29th Sept. 1875.	Remarks.
India-rubber to india-rubber	No. 1	Nov. 17th, 1873	Over 100,000 millions	Over 100,000 millions	Over 100,000 millions	
do.	2	do.	„ 100,000 do.	13,000 do.	3,750 do.	Core attacked by a borer-perforation did not reach conductor
do.	3	May 29th, 1874	„ 100,000 do.	Over 100,000 do.	Over 100,000 do.	
do.	4	do.	„ 100,000 do.	„ 100,000 do.	„ 100,000 do.	
do.	5	do.	„ 100,000 do.	„ 100,000 do.	„ 100,000 do.	
do.	6	do.	„ 100,000 do.	„ 100,000 do.	„ 100,000 do.	
do.	7	do.	„ 100,000 do.	„ 100,000 do.	„ 100,000 do.	
do.	8	do.	„ 100,000 do.	„ 100,000 do.	„ 100,000 do.	
do. Cut out of cable on board the "Amber-witch"	9	February 1873	Cut out of india-rubber cable in tanks, January 1875	„ 100,000 do.	„ 100,000 do.	
do.	10	do.		„ 100,000 do.	„ 100,000 do.	
do.	11	do.		„ 100,000 do.	„ 100,000 do.	
do.	12	do.		„ 100,000 do.	„ 100,000 do.	
Common sealed ends of gutta-percha core	No. 1	July 1874		„ 100,000 do.	„ 100,000 do.	
do.	2	do.		„ 100,000 do.	„ 100,000 do.	
do.	3	do.		„ 100,000 do.	„ 100,000 do.	
do.	4	do.		„ 100,000 do.	„ 100,000 do.	
do.	5	Jan. 21st, 1875	Over 100,000 millions	„ 100,000 do.	„ 100,000 do.	
Gutta-percha to gutta-percha	No. 18	Mar. 17th, 1875			„ 1,200 do.	Two coatings only
do.	19	do.			„ 700 do.	
do.	20	do.			„ 1,250 do.	
do.	21	do.			„ 150 do.	
do.	22	do.			„ 420 do.	
do.	23	do.			„ 730 do.	
do.	24	Not known			„ 1,540 do.	Cut out of old cable in stock
do.	25	do.			„ 400 do.	
do.	26	do.			„ 115 do.	
do.	27	do.			„ 50 do.	

HENRY C. MANCE.

Henley's works. During the interval it had been frequently recoiled, but was never laid. When I again tested it, I was rather surprised to find the resistance of the dielectric so low; but investigation proved the cause to be in the joints, and I attribute the cause of their low resistance to their having been kept in a dry state for four years.

With regard to old core, there is no difficulty in joining old and new, provided care be taken to thoroughly clean the surface; not with a solvent of gutta-percha, as is usually done, but by shaving off the surface of the gutta-percha with a "trimming knife;" by this means perfect adhesion between the old and new is obtained.

One question raised in the paper is, "Will joints made in old cores stand as well as new, and will they stand at the same resistance for twelve months?" I do not think any joint will test, after twelve months' immersion, the same electrically as when first made. I have the record of one or two experiments on the subject which may be interesting. In 1871 I got nine of our best jointers to make twelve joints each in thirteen yards of a small core. These lengths were marked respectively A, B, C, D, E, and F. Each length was tested separately by the accumulative system, after twenty-four hours' immersion in water, kept at an uniform temperature of 75° Fahr. The standard was thirteen yards of the same core, but containing no joints. The results were as follows:—

Standard	=	1·000
A	=	1·343
B	=	1·256
C	=	1·228
D	=	1·371
E	=	1·314
F	=	1·371

The usual standard is, that the leakage from a length of core containing a joint shall not be greater than the leakage from twice the length of the same sized core containing no joint; consequently this test was considered satisfactory.

I tested these joints under precisely the same conditions again, after having been immersed in water kept at an uniform temperature of 75° Fahr. for three months. The test was then—

Standard	=	1·000
A	=	1·333
B	=	1·361
C	=	1·305
D	=	1·361
E	=	1·305
F	=	1·277

They were again tested after they had been immersed five months at an uniform temperature of 75° Fahr., with the following results:—

Standard	=	1·000
A	=	1·625
B	=	2·250
C	=	1·350
D	=	1·300
E	=	2·000
F	=	1·250

I have not tested these joints since until yesterday. They have been immersed in water, but not at an uniform temperature, for four years. The results of the tests were as follows:—

Standard	=	1·000
A	=	81·200
B	=	58·000
C	=	41·500
D	=	2·500
E	=	72·000
F	=	3·100

It is well known that the electrical resistance of gutta-percha increases by age, and in four years the resistance of the thirteen yards used as the standard has increased threefold.

There is another very important suggestion made in the paper, which I was sorry to see. The author of the paper throws out an idea which, if adopted, would carry us back to where we were twenty-five years ago. I allude to where he suggests that joints made quickly stand better than those with which pains are taken. That is contrary to all my experience. In fact I do not think too

much care can be taken in the making of a good joint, especially in the small-sized cores now generally used for subterranean and submarine lines, where the length does not exceed 1,000 miles. In cores the size of those in the Atlantic and Persian Gulf cables there ought to be no difficulty in making good joints, if an experienced man is employed. It is a curious fact that the same jointer seldom makes two joints that will test the same electrically after they have been made a few days. Jointers who accompany cable-laying expeditions, or have been otherwise engaged from the core factory, after their return are incapacitated for a time from making a joint that will pass the electrical standard. I think that is to be accounted for by the fact that clean hands are absolutely essential to the making of good joints. In the core factory their daily occupation is joint-making, but when from home occasions often arise when months may elapse before they are required to make a joint, and in the interim the jointer is employed in work which certainly does not improve the hands for joint-making. Attending to and cleaning batteries, for instance, may be mentioned as one of the objectionable occupations to which a jointer should not be put.

Some men are physically incapacitated for joint-making, especially those who have warm moist hands. As far as my experience goes, there is one important fact which is very encouraging, that is, that a *good* mechanically made joint, although it may alter electrically, will never fall so low in its resistance as to interfere with the working or the duration of the life of a cable. I have by me a coil of bad joints, that is to say, all the rejected joints that have come under my notice. I have had them joined together, and I think at the present time there are 288 joints all told. Some of these joints have been immersed for nine years, during which time they have been subjected to every kind of hard treatment. Quick reversals, from a battery of 500 cells, have frequently been employed for several months at a time with a view to break them down, but without success, and the present resistance of the entire length is 24 megohms. Not very long since a portion of the cable laid between Malta and Alexandria in 1861 was recovered, and in the core taken from the same I found two joints, one of which had the mark of the jointer at the cable factory, and the other that of

the core factory. On comparing them with the present electrical standards, the tests were as follows:—

Standard = 1·0
Core factory = 2·7
Cable ,, = 40·0

Considering that those joints had been immersed over twelve years, I consider the tests satisfactory. There is rather an important question in connection with joint testing, which I think is worthy the attention of telegraph engineers. I refer to the standard hitherto used at the cable factory. I have already stated that the standard at the core factory is, that the loss from one yard of the core containing a joint shall not be greater than the loss from two yards of the same core containing no joint. The standard at the cable factory is the same. Now, suppose a joint has passed the test at the core factory, and many weeks elapses before the same joint is again tested at the cable factory, and then the loss is found to be equal to four yards of core, would it not be better to pass that joint at a lower standard than reject and trust to a new one made in the cable factory, and almost immediately passed into the cable, where no further opportunity is given of again testing it? In conclusion, I hope Mr. Mance will erase from his paper that part in which he suggests that joints made quickly are better than those on which time and care have been bestowed.

The PRESIDENT: You have sent some specimens, can you inform us what this is?

Mr. SMITH: The coil of wire which you hold in your hand is a portion of the gutta-percha-covered wire which was laid by way of experiment from Dover to Calais in 1850. As I was engaged in the manufacturing and laying of that, the first submarine line, I have always taken great interest in anything connected with it, and, having heard that a fishing-smack had recently picked up a length of the same and brought it into Dover, I applied to the Submarine Company, and their engineer kindly sent me the length now before you. Its present electrical resistance is 645·3 megohms per knot after one minute's electrification, after twenty-four hours' immersion in water kept at an uniform temperature of 75° Fahr. Considering that that length has been one of the waifs and strays in

the English channel for twenty-five years without any protection, I think that its present condition speaks volumes for the durability and general suitability of gutta-percha for submarine telegraphy. The other specimens simply show the crude way in which the joints were made twenty-five years ago compared with the present system.

Mr. T. T. P. B. Warren: The efficiency of a joint, whether in india-rubber, gutta-percha, or between the two materials, frequently depends upon the jointer more than on the materials themselves. A man of a nervous temperament should never be allowed to make joints, for a fidgetty over-cautiousness in joint-making will generally end in a bad result.

The remark made by Mr. Mance that "there is a point beyond which manipulation and the use of the spirit lamp will do positive harm" is undeniably correct.

If the hands of the operator are liable to excessive perspiration it is impossible for a joint to be made having even a fair degree of insulation. I speak here from experience on india-rubber, whether in its vulcanized or unvulcanized forms, and I should infer that the kneading of gutta-percha with damp hands would lead to a similar result. A man whose hands perspire freely should never on any account be employed as a jointer. I have seen instances where excessive zeal, in endeavouring to keep the hands clean by washing, has resulted in defective joints.

In the tropics and during hot weather these remarks cannot be too forcibly impressed upon those having the charge of cable-work. In fact I feel more disposed to attribute the defective jointing in gutta-percha, and between gutta-percha and india-rubber, to this cause than to the materials. Mr. Mance says that he found the joints which were more hurriedly made test better than those on which a more extended manipulation was given. I can myself quite conceive that such a result should follow, as the materials for jointing should be handled as little as possible.

We should not forget that naphtha or spirit of wine when burning produces an enormous quantity of aqueous vapour compared with the quantity of spirit consumed, and in which the ends of the gutta-percha cores are heated when prepared for jointing. This

is liable to become at once a fruitful source for defective gutta-percha joints. It can be easily remedied, and, I have no doubt, manufacturers of gutta-percha cores have their remedies for this evil. Mr. Mance says that the insulation of the "Fao-Bushire gutta-percha cable" is higher now than it has ever been, which is a clear proof of the durability of the joints when made in England, and at the core or cable works.

I have tested several gutta-percha joints, and I must say, in fairness to this material, that I have never met with anything approaching the disastrous results met with by Mr. Mance, and I may apply the same remark with greater force to joints made between india-rubber and gutta-percha.

Taking the first four joints in Mr. Mance's tables for observation, if I found that a jointer could make joints, with such wide results as indicated in the table, I should say something must be wrong, for clearly, if one joint can be made to keep right, why not the remaining three?

As regards the permanency of joints made in gutta-percha cables after being laid some time, I believe there is an opinion that they cannot be made to stand so well as when made in recently manufactured core or cable. My experience on this point is limited only to a few cases. We had an occasion during the past summer to pick up a cable belonging to the Cuba Submarine Telegraph Company, running from Batabano to Santiago, for the purpose of attaching shore ends into Cienfuegos; the jointing was performed without any difficulty between the old and new gutta-percha, and, to all appearance, seemed as perfect as any joint I have seen. As an experiment, and partly to keep the jointer employed, I had three joints made in the new core, and one between a piece of old and new core. By testing with 500 cells on a marine galvanometer, one division of the scale of which would represent a resistance of nearly one thousand megohms, none of them gave the slightest deflection. After keeping them for a few weeks, they were cut open, when it was found impossible to separate the old from the new materials. The man who made these joints was a good india-rubber jointer, with some years' experience, but who had made only a few gutta-percha joints for practice before joining up the cables.

Instead of washing the hands with soap and water it is far better to clean them with a little naphtha; either wood or rectified coal tar will do, although preference in india-rubber jointing should be given to the latter; the evaporation keeps the hands cool and free from perspiration, whilst its vapour has no effect on the insulating materials. I adopted this plan in the tropics and West Indies, and had, in consequence, but very few occasions to order a joint to be remade.

The proper routine in selecting a jointer is, I fear, rarely followed, chiefly because a man is marked out for distinction on the principle of favouritism, and, whether fit or not, must be made a jointer. Although I have seen and tested some thousands of joints in india-rubber in its various forms, I have seen but a very small number defective. This I attribute to the method of selection for jointers. No man should be allowed to make joints in cores or cables without his first undergoing a fairly experimental training: this first operation should be confined entirely to temporary work, each joint being well marked.

These little preliminaries to joint-making may appear somewhat fastidious, but it must be remembered that a jointer, although so much depends upon his work, is generally, and I fear in most cases on repairing-ships, a man whose knowledge is the result of experience, and attained strictly by rule of thumb.

It is not a good plan, in arriving at the worth of your jointer, to tell him to make a few joints specially for testing; it is far better to cut out a few from the drums of core now and then at the core works, for in this way you get a test of his actual work.

Too much care cannot be taken in the selection of a jointer. The electrician in charge of an expedition ought to know not only the method of joint-making as well as the jointer, but he should be able, at a moment's notice, to extemporise the minutiæ required by unforeseen circumstances and conditions.

Presuming, firstly, that we have found a man, the model for a jointer, instead of allowing him to make a joint as occasion requires, he should be directed to make a few now and then, when unoccupied, not so much with an idea of keeping his hand in, but as a test of the suitability of his jointing materials and tools, that

no deterioration has arisen, and that his stock is as it should be, and that his tools and appliances are ready at hand, if required, on an emergency.

The preservation and protection of the jointing materials, especially at sea and in warm climates, is not a matter which should be left entirely to a jointer, unless he is a man of good experience and trustworthiness.

I am inclined to think that jointing materials are not always sent to sea as they should be, or, if sent properly and securely packed in air-tight cases, are not preserved with the care required afterwards, the result being the manufacture of a defective joint due to partly oxidised or decayed materials. I do not think, however cautiously packed, that any raw insulating material should be depended upon after being kept in a warm climate over eighteen months or two years. I have no doubt that core manufacturers would willingly take back the refuse jointing materials, as they could be used up for other purposes besides insulating.

Apart from the causes already pointed out as likely to produce bad joints, there is one arising purely from the effect of surface hydration,—unless extreme care is used, a bad joint follows through the want of cohesion at the extreme ends of the new materials. This defect is more likely to be met with in india-rubber cores than gutta-percha, in consequence of the surface becoming more hydrated. A gentle heat will frequently remove this; and if the core has been for some time in sea-water it should be previously washed in a little condensed water and dried.

There can be no doubt that the diminished chance of getting defective joints in india-rubber as compared with gutta-percha is due to the heat to which the india-rubber must be submitted to ensure its consolidation, and which, although it does not melt it, softens it so as to be compressible into an uniform mass under a moderate pressure, conditions which would involve the disturbance of the conductor from its central position in gutta-percha cores.

I met with a very singular case some time ago, in which a man who had hitherto made excellent joints suddenly produced joints of a very indifferent quality, and, although I had several made

for testing, I found them all more or less defective. I found this was due to the man's suffering from bad health; as he recovered, his joints became as good as ever.

I now come to another consideration, namely, the testing and inspection of joints.

Every joint should be examined and tested whenever practicable. The testing of joints, even if the result be satisfactory, should never be allowed to dispense with the inspection, for it must be borne in mind that the time allowed for a joint to stand in water, more especially at a cable factory, or on board ship, is far too short to allow the water to get through even a very defective joint. It is well known that a joint may test very well some hours after it has been made, but, after some time it may become a source of vexatious annoyance. Hence it is preferable to test joints at a cable factory, instead of when made at the core-works, for the joints, in case the former are frequently some weeks under water before testing.

A sound joint, when cut open, should, as far as the insulator is concerned, be perfectly compact, free from air-bubbles, and its several coatings, no longer retaining their individual nature, should so adhere to each other and to the parts joined as to be inseparable at the original points of junction. On the outer surface, the ends of the coatings should so unite with the older materials as not to yield when the finger-nail is forcibly applied to the junction. Any joint not complying with these conditions should unhesitatingly be rejected.

With gutta-percha cables this examination can be carried out at once, but with vulcanised joints it is necessary to remove the covering or binding of cotton, and this can be safely done only when the joint is hot. The joint being found satisfactory, it should be well wetted, lapped again with felt, and soaked a short time until cooled before covering up into cable.

I have extended the same examination to the few joints I have had to deal with in gutta-percha, the only difference being the use of ice to cool the joint before covering up into a splice. With a rigid material, I should fancy a gradual cooling would be preferable to rapidly reducing the temperature, as tending less to an unequal contraction of the coatings.

The testing of joints by the accumulation method, with the condenser or quadrant-electrometer, is too well known to require any description here. On board ship I have employed a large form of Thomson's portable electrometer, and, although it is extremely troublesome and involves delay, I would nevertheless recommend that every cable expedition should carry one of these very useful instruments. I shall have occasion to speak of this instrument at a future time.

One of the most important points at sea is to economise time over a joint, but this should never be attempted at the sacrifice of efficiency. A skilful jointer who keeps everything ready at hand will save several minutes over a joint—a matter of great importance with vulcanised joints if working in a tide-way or the threatening approach of a rough sea.

When a defective joint is met with, it should be carefully tested in this way: First, well clean it with water or naphtha, unless there is fear of its concealing the defect. Connect it to an electrometer, and charge it and the entire joint by grasping the hands around it, then let the outer surface be freed; the operator then tests the piece of core on one side of the joint, taking care not to touch the insulator junction, by gradually wetting with a camel's-hair pencil, which is in communication with the ground. The other side is next dealt with in the same way, when, if the electrometer shows no fall of tension, the fault is in some part of the joint; the junctions are next tested separately by passing the pencil carefully around. If the first one tested gives a faulty indication, recharge and examine the other, for one end may be defective and the other all right. Afterwards carefully dry the parts already found defective, and proceed in the same way, testing the joint itself; this is best done from the middle, going carefully round the core towards one of the ends or junctions. This done, proceed towards the other junction.

This method of testing a defective joint is invaluable, for it not unfrequently happens that a faulty joint is the result of a local flaw. A galvanometer will do equally well for this test, unless the loss from the joint is very small.

In conclusion, I would remark that manufacturers of core would do wisely to give to the owners of submarine cables notice of any

improvements which might arise in the manufacture of joints or the preservation of jointing materials.

I would suggest as an experiment the binding of gutta-percha joints tightly with canvas whilst soft, as I believe that in india-rubber joints the binding, when the joint is heated, is of inestimable importance.

I think this paper of Mr. Mance's is a very important one, not only to the Society of Telegraph Engineers but to the proprietors of submarine cables.

The further discussion of the paper was adjourned until the next meeting.

The Thirty-ninth Ordinary General Meeting was held on Wednesday, the 24th November, 1875, Professor ABEL, Vice-President, in the Chair.

The SECRETARY having read a letter which the Council had received from the son-in-law of the late Sir Charles Wheatstone (Mr. Sabine) in acknowledgment of the especial expression of sympathy and condolence addressed by the Society to his family,

The CHAIRMAN said: It is now my duty to invite members to resume the discussion on Mr. Mance's paper "On the respective Merits and Durability of India-rubber and Gutta-percha Joints," which was read at the last meeting, and the discussion on which was adjourned till this evening. Perhaps Mr. Bell will oblige us with some remarks on the subject.

Mr. A. BELL (responding to the Chairman): I have very few remarks to offer, but during the last few years I have noticed the decay that is often found in gutta-percha wires and joints when removed from street and underground lines generally. I have to-day had an opportunity of seeing some joints which were made ten or twelve years ago, at the time when the Electric Telegraph Company used thick wire No. 3, and in some cases No. 1 gauge. Some of these joints are known by those who made them from the marks upon them, and from that circumstance the period at which they were made has been fixed at from ten to twelve years ago. The wires from which these joints were cut have been several times in use; perhaps they have been three times drawn out of the pipes and then used again, so that they have had a great deal of pulling about, and yet these joints are still in good condition. Now that we are using a smaller gauge of wire, No. 7, the joints are found to be not so good. The conditions affecting durability are not so favourable as in the case of submarine cables, which have a core of a much larger diameter, and are less subject to extreme variations of temperature. Mr. Willoughby Smith alluded to the overheating of

the joints. I find from inquiry that a source of very many faults is from the over-use of the spirit-lamp or dressing-iron; and in that case we find the joint becomes brittle after a time. A jointer, in whom I have great confidence, remarked to me that wherever the spirit-lamp is used too long the joints become bad in a very short time. If we could find a way of making the joints of these small wires perfect it would be a great gain.

In connection with joints I may state that about a year and a half ago Mr. Culley requested me to make some experiments with paraffin. Different forms were tried, but the form in a zinc tube was found best, and I have brought a few joints here which were made at that time and still test good. I have tested them by the accumulation and the direct test, and I find there is no more leakage than from a similar length of wire of the same size without a joint. I have since made other experiments, and I find it is necessary to be careful with regard to the temperature at which the paraffin is used. It should not be used at a heat much above the melting point or there is risk of damage to the gutta-percha. There may also be some risk from the contraction of the material at a low temperature. These joints which have been lying for about a year and a half have never been subjected to a low temperature. If it were reduced to freezing point further contraction might take place, and this would probably cause fracture of the paraffin, resulting in a slight leakage. I think the paraffin joint is worthy of a trial where it can be done. I have tried ozokerit, but though it sticks well it is not so fluid, and on account of its having a higher melting point it is more likely to injure the gutta-percha and thus make an imperfect joint.

Mr. T. E. Phillips: I cannot tell you anything very new, but can confirm what Mr. Warren says as to the difficulty of testing india-rubber joints which have been soaked in any kind of water. Not long ago I tested the joints of an india-rubber core which had been for some time in water, and I found it impossible to get an insulated trough owing to the leakage along the outside of the core. I scraped the core on each side and dipped it into boiling paraffin, and still could not insulate the trough; the only way I could succeed in doing so was by snipping the core all round to the depth

of about 1-16th of an inch. The water had evidently penetrated a considerable depth into the core.

With reference to gutta-percha joints I have not much to say, except that it has occurred to me that they might be made with advantage with a jacket containing superheated steam, so as to do away with the spirit lamp, which seems very dangerous.

Mr. WARREN spoke of the hydration of the surface. I have found no difficulty in testing on account of this. The only precaution is to heat the surface of the core with the tool or spirit lamp. When the core is soft we wash the surface, and a few moments' application of the heat will render the surface perfectly secure for testing.

The CHAIRMAN: The spirit lamp is to be cautiously applied to avoid injuring the surface.

Mr. WARREN: A man would have to keep a spirit lamp to a vulcanised material for a considerable time before it would produce any effect.

MAJOR WEBBER: Mr. Bell has referred to the joints in the underground telegraphs, which form part of the Postal Telegraph system. It struck me whether mechanical joints had been used to insulate the joints which we see in the flush-boxes placed in underground lines Mechanical joints have the advantage that they afford ready accessibility to the wires, and, if they give perfect insulation and are easily opened, it seems to me that such a mechanical joint as was suggested some years ago by Sergeant-Major Matheson, of the Royal Engineers, would be a simple one to use on wires passing through flush-boxes in underground telegraphs; and by their use the wires would be accessible for any purpose. I remember one of these joints being shown to Mr. Culley, and it was suggested to him that in cases where it was desirable to test wires frequently, and where a jointer could not be procured to make a joint good again after it was cut, these joints would be excessively useful. Mr. Culley had one tested, and it gave perfect insulation when immersed; but unfortunately the circumstances under which the test was made were not favourable for giving a good impression of the joint, or producing good results, because by a curious coincidence it was placed over a piece of gutta-percha covered

wire which had not been cut at all, and insulation was given by the perfect insulation of the wire, and not by the joint. I do not know whether any further test was made with these joints, but perhaps Mr. Bell can tell us.

Mr. Bell (in reply to Major Webber's question): Mechanical joints were used between London and Manchester and Liverpool by the Electric Telegraph Company in 1853, in laying an 8-wire line of gutta-percha wire. They were placed in earthenware boxes set in the ground, in the line of the pipes, at certain arranged distances apart. Some gentleman connected with the Company designed the coupling, which consisted of a small strip of copper, about half-an-inch wide, embedded in a half-circular core of gutta-percha, and the same on the other side. At the bottom of the cores there was provided a short piece of wire for connection with the ends of the wires in the pipes; there was a dowell to keep the half cores straight, and a coupling-screw to fasten the copper strips together, and the whole thing was kept in position by a conical cap pushed quite down over the core. Mr. Preece may be able to say how long it was before these underground wires were lifted, but how long they remained I am not myself able to state. The joint was uncoupled when necessary for the purpose of making tests. It might cause leakage, unless something was stuffed into the box or cap to prevent moisture getting in.

Capt. S. Anderson, R.E.: When this mechanical joint was first made in our workshops at Chatham, about four or five years ago, it was done at a cost of about 1*s.* 6*d.* each. We made a great many experiments at Chatham, in most cases with inexperienced men. We found that a man, after being shown once or twice how to couple the joints, could be relied on to make one which stood the insulation test under a pressure of water of six, seven, or eight feet. These joints were subsequently immersed in the river under thirty-six feet of water, and were extremely good. Latterly, the expense of these joints caused them to be put aside, and all the joints in submarine experiments were made entirely with india-rubber tape and india-rubber solution, lapping them carefully over, having previously put over each a piece of vulcanised india-rubber tubing, while above it the india-rubber was securely

lapped with twine. A joint of that kind was made quickly and efficiently, and some of them remained eight or ten months in the river. I may mention, that to take the strain off the joint there was generally a hitch put in the wire or cable, so that in pulling the strain was borne by the hitch. I have seen these joints opened after many months, and I have remarked that the india-rubber solution and tape round the joint were perfectly jammed with the material of the core, and the vulcanised india-rubber tubing was also in perfect preservation. In fact, although this joint was only put down for temporary purposes, it really answered for a permanence.

Mr. W. H. Preece: It is not often that we find an inventor so modest in propounding his own ideas as Mr. Bell has been to-night. I was in hopes that he would have given us a short lecture on these new joints of his, because in point of fact they are a complete revolution in our ideas of making the joints of gutta-percha covered wires. Of course there are joints and joints. A facetious friend near me suggests there are hot joints and cold joints; but there are submarine joints and underground joints, which are two totally different things. We have heard about the care required for submarine joints both from Mr. Willoughby Smith and Mr. Warren; but whatever care is exercised in making submarine joints it is almost impossible to do the same in making those joints which are found in our underground system and in that system of gutta-percha covered wires which passes through so many cities and towns in this country.

Now, although to many people the subject of joints may appear a very uninteresting one, to those who are occupied in the maintenance of telegraphs there is no subject more interesting and none which has more occupied the thoughts of telegraph engineers. In the streets of London in particular we are very much troubled with bad joints in busy thoroughfares where crowds of curious people are constantly passing. At those times the jointer is interfered with in his work to such an extent that it is almost impossible for him to make a joint in a way in which a proper experienced jointer can make it in a submarine cable or in the warehouse; and, owing to the repeated and numerous failures which we meet

with in these joints, the thoughts of Mr. Bell and many others who are engaged in the maintenance of telegraphs have been devoted to endeavour to improve the mode of making them. We have not yet succeeded—joints still go bad; and twelve or fourteen months ago Mr. Bell suggested that instead of making the joint with layer upon layer of gutta-percha we should try what could be done with paraffin. Mr. Bell made some joints, and from our experience of them up to the present they have turned out very well. These three particular joints were made merely as specimens for inspection. They have been knocking about at the Gloucester Road Stores, and within the last few days have been carefully tested by the accumulation and other tests and found to be as perfect as on the day on which they were made.

I may in the first instance explain that the two ends of the gutta-percha wire are brought through two holes bored through a small piece of wood, and the copper wire being exposed is simply twisted, lapped up, and soldered. This zinc tubing is then put over and then melted paraffin is poured in. In order to give this system a thoroughly good practical test—for in all these questions the real test is in practice—at Mr. Culley's request I had several of these joints applied to the street-wires in Bristol. Forty-five wires are laid through the streets of that town; but, instead of adopting Mr. Bell's original idea of using zinc tube, we took a piece of birch boarding, 3 inches by 2, in which we bored ten holes, till they came within half-an-inch of the bottom of the wood. The holes were five-eighths of an inch in diameter. The wires were then brought through, twisted, soldered, pulled back again, and then the melted paraffin was poured in. Well, this was carried out, and at first it answered perfectly, but after a time fault after fault occurred, and careful tests showed them to be at the very spots where these joints were made; so we were obliged to take them away and carefully examine the cause of failure. You often learn a great deal more from failure than from success, and the very failure of these joints has tended really to impart confidence in the use of paraffin for this purpose, because the failure has been proved to be attributable to the simplest possible causes. In point of fact, the cause of failure was owing to the applica-

tion of too great a heat in melting the paraffin; the gutta-percha itself was burnt, air-bubbles formed inside the gutta-percha, and by making it too soft the wires had become eccentric, and the copper wire was found to be in contact with the wood sides. Series of experiments have proved that these faults are easily remedied—first, by applying a heat to paraffin under 200°. The melting or liquefying point of paraffin being about 180°, a heat of about 200° will bring it into that plastic liquid state in which it can with ease be poured into these tubes or holes, and so form a solid mass round the joint. The great defect with the pieces of wood was the slowness with which the paraffin cooled, so that we have abandoned the idea of using wood blocks, and we have determined to substitute Mr. Bell's original idea of employing zinc tubes. The experiments are going to be repeated at Bristol, and the form we intend to adopt is as follows:—At the extreme end, the two wires are brought up and the copper wire is twisted up and soldered; a tube is placed over that, and then paraffin, which has been liquefied at a heat not exceeding 200°, is poured in. The joints when made in this way quickly solidify, and I have had some specimens brought here which can be inspected after the meeting. This is, as I have said, simply an experiment, which up to the present has hitherto failed, but its very failure gives us hope of ultimate success; and having described the experiment that we are about to make, perhaps on a subsequent occasion, when the subject of joints is again brought before the Society, I may have the pleasure of giving you the results of our continued experience at Bristol.

Mr. Willoughby Smith: I do not know that I have any more to say on this subject. With regard to the mechanical joint referred to by Mr. Bell, I don't see what advantage there would be in again using such a joint for subterranean lines. It was simply an easy arrangement for disconnecting wire, for testing and other purposes, and necessitated two joints being made when only one is now required. With regard to the jointing of the subterranean wire, if I may judge from what I saw at one of the test-boxes in the City a few days since, no improvement has been made. In fact, the work was being done in a more dirty and slovenly way than ever it was.

Mr. Fleetwood: With reference to the mechanical joints described by Mr. Bell, I remember, soon after the transfer of the telegraph to the Post Office, the wires of the late London and Provincial Company, particularly in the City, were continually getting wrong. When tests were made it was found that the whole of the wires of certain sections were bad. Some of the flush-boxes were buried, and when they were opened we found the wires connected by the mechanical joints; when these were cut out many of the wires proved to be good.

Many of these joints were taken out, and wires, which were reported to be useless, brought into use and still remain good.

Some of the wires in Cheapside, I may say nearly the whole of them, were bad between the west end corner of Cheapside and the corner of the New Post Office, and had to be replaced by a new cable; probably these faults were owing to a box being buried under the asphalte.

As regards the joints in the street work of London, I will not say they are as good as they might be, or ought to be, but I do not think they deserved to be condemned to the extent they have been. I have been in the habit of testing street wires, and have found the average insulation is something like 12 megohms per mile; this is quite equal to work the lines. The insulation of the overground lines is far below this. We endeavour to keep the underground wires to this standard by continually testing. When we see a wire is getting low in insulation a loop test is taken and the fault traced. Sometimes it is a bad joint, but it is not so often in the joint-boxes as in the pipe, and is in that case probably caused by lightning.

I quite agree with Mr. Willoughby Smith that the present mode of making joints is a very rough one, and that very little improvement has been made. I hope what has been said here will lead those in power to have more care exercised. When the same care is taken in underground lines as in submarine, I have no doubt our wires will test much better than they do now.

Major Webber: I may remark that the condemnation of the present system of jointing with gutta-percha wire, has I think arisen mainly from the fact that an enormous number of joints are

made by men incompetent to make them properly. In that way the system of jointing to which Mr. Willoughby Smith refers has been brought into discredit, because the joints which have turned out bad have been supposed to be made in that way, and were not. I have seen some hundreds of joints cut out which have been made in gutta-percha-covered wires at various times by so-called jointers of different Telegraph Companies. I think the system of making joints under Mr. Bell and Mr. Fleetwood is the best of anything yet employed in land wires, and the failures have been chiefly in cases where it has not been possible to send a jointer to make the joints, but a mere tyro, who thinks it is enough to wrap a piece of gutta-percha tissue over the wires, heat it, and press it on. This has brought them into discredit. I still think Sergeant-Major Matheson's joint is the best; it is most easily applied, and gives insulation equal to the best joints in the London street wires. Of course, it is desirable that a better system of jointing should be found out, but I think the improvement will be in the direction described by Mr. Preece—that is, a joint made in a more simple manner, and it may be possible to make it by means of men who do not require that excessive co-efficient of dryness of hand and good health which Mr. Warren has mentioned as being necessary to produce a good joint that will last as many years as the percha with which the wire is covered.

Mr. A. J. S. Adams: Some years ago I noticed a jointer at work under a small portable tent, whereby the gutta-percha wires and appliances were kept entirely free from wet and grit. This I think could be easily done in a great many cases, no doubt much to the improvement of the work, and the joints thus made would be more perfect than could be made under the less favourable circumstances.

Captain McEvoy: I have had some little experience in joint-making of telegraph wires and for the most part with mechanical joints. I am myself the inventor of three or four different forms of mechanical joints, and I hope I shall not be considered egotistical when I regard them as an improvement upon Sergeant-Major Matheson's joints. They are in the same direction. It appears to me that the objection to mechanical joints in their application to land-

wires would be their expense. The mechanical joints I refer to are not yet produced at a cost of less than 2*s*. each, which is about the lowest price at which we have furnished them, and then only for very small wires. The expense is, I think, the principal objection against the mechanical joints. In other respects they have many advantages over either the solution joints, which I believe apply only to india-rubber-coated wires, or the common means of cementing the gutta-percha in the case of gutta-percha joints.

The CHAIRMAN: We owe our best thanks to Mr. Mance, not only for the interesting paper he has communicated on this subject, but also for the interesting and instructive discussion which that paper has elicited. I have little doubt this subject (than which perhaps there is none more important in connection with telegraphy) will benefit very considerably by the remarks that have been elicited. I have been, personally, much interested in what Mr. Preece and Mr. Bell told us in regard to the employment of paraffin as a rapid and very effectual method of jointing, and I look upon it myself very happily. Experience in the use of this material for jointing would seem to point in the direction of not using a higher temperature than is necessary; and it is to be hoped that considerable changes of temperature will not affect this description of jointing. Ozokerit, which is a natural form of paraffin, melts at a considerably higher temperature than ordinary paraffin, and may be somewhat less ready of application, though I do not conceive any practical difficulty could arise in this respect. I may mention that I can confirm what Captain Anderson has told us, first, with regard to the efficiency of the mechanical joint constructed by Sergeant-Major Matheson, and since improved upon by Captain McEvoy, and also that joints prepared with the aid of india-rubber solution are equally applicable to gutta-percha joints.

The vote of thanks to Mr. Mance was unanimously passed.

The following papers were then read—

EXPERIMENTS CONDUCTED FOR THE PURPOSE OF ASCERTAINING WHETHER THE TEREDO BORER PREFERS GUTTA-PERCHA TO INDIA-RUBBER.

HENRY C. MANCE,

Government Telegraph Department, Persian Gulf.

Having received instructions early in 1874 to submerge some pieces of cable in the Kurrachee Harbour to ascertain, if possible, whether india-rubber was as liable to the attacks of borers as gutta-percha, the following experiments were made by me for the purposes of obtaining the desired information. The results are communicated, by permission, for the information of the members of the Society of Telegraph Engineers. An inspection of the portions cut out of the experimental pieces will, I think, be found very interesting.

The pieces laid in the harbour for trial were as follows:—

1 length of bare gutta-percha core.

1 length of bare india-rubber core.

1 length of Persian Gulf gutta-percha cable.

1 length of Persian Gulf india-rubber cable.

One of the sheathing-wires had been removed throughout from each of the pieces of cable, so as to allow of the borers reaching the india-rubber or percha through the intermediate serving without difficulty.

The length of each piece was nearly 200 yards, and they all tested good before submersion. They were laid in the immediate vicinity of each other, and, to make the conditions of trial as equal as possible, the bare percha core was lashed alongside the piece of gutta-percha cable; the bare india-rubber core was also lashed to the india-rubber cable.

On recovery after a submersion of nearly ten months, the only piece which tested good was the length of india-rubber cable; the other three were bad from the following causes:—

The core of the gutta-percha cable was riddled by borers, the

conductor being exposed in most cases; there were probably a hundred perforations in this piece, the teredo usually approaching the core through the space caused by a missing guard.

The length of bare gutta-percha core laid alongside the foregoing piece had but *five* borer holes in it; it was otherwise uninjured, as the barnacles grow *round* gutta-perch core, adapting themselves to the form of the core without cutting into it to any dangerous extent.

The piece of bare india-rubber core had not a single borer perforation, but was found to contain a number of very remarkable faults towards the end which had rested in shallow water. The rubber was notched as neatly and regularly as if the injury had been done with a sharp knife, but in a few instances the piece of india-rubber was found remaining in the notch, the conductor being in nearly every case exposed. In the neighbourhood of these faults the core was thickly covered with barnacles, and it is possible that these peculiar injuries have been caused by some marine animal feeding on the barnacles, which attach themselves to the core, and which I understand are greedily devoured by fish. The barnacles adhere so tightly to india-rubber that it would be difficult for a marine animal to remove them without tearing away a portion of the rubber.

Bare india-rubber core is more susceptible to injury from barnacles than bare gutta-percha. As the barnacle grows, the base of the shell cuts into the yielding rubber, a second shell attaches itself to the side of the first, the growth of the circumference proceeds towards the conductor, and eventually the sharp edge of one of the cluster reaches the copper wire.

The piece of india-rubber cable tested perfect; but on examination, after stripping off the guards, about a dozen marks were discovered, showing that the borers had been at work. In no case, however, had the teredo succeeded in penetrating to the conductor. Many of the borers in the gutta-percha were still alive; but, not having been fortunate enough to secure a living specimen in the india-rubber, I am unable to say if the teredo was still at work at these marks.

Briefly to sum up the results of the experiment:--

Gutta-percha or india-rubber core, without any covering, is not so liable to damage from borers as core partly protected by imperfect sheathing.

Bare india-rubber core is liable to damage from barnacles to a greater extent than gutta-percha, in consequence of its yielding nature; but, on the other hand, the teredo will attack bare percha and usually leave bare india-rubber untouched.

Gutta-percha cable, the sheathing of which is much corroded or imperfect, so that the guards do not rest closely together, is utterly unfit for localities infested by the teredo.

Although it appears clearly proved that india-rubber core, either unprotected or protected, is very much less liable to the attack of the teredo than gutta-percha, the experiment is not sufficiently conclusive to show whether borers would succeed in perforating india-rubber through to the conductor of a cable, if submerged for a longer period than ten months. To decide this point a short piece of old india-rubber cable with defective sheathing has now for some time been lying in the Kurrachee harbour.

Finally, only cable of which the sheathing is perfect should be laid where there is a chance of its being attacked by the teredo, and, so far as our experience goes, india-rubber is less liable to injury from this cause than gutta-percha.

CABLE BORERS,

By G. E. Preece.

The following additional remarks have been made, in order to supplement the information contained in Mr. Mance's paper, with particulars of the damage done in other parts of the world by marine life to submarine cables.

The first appearance of any damage done to a submarine cable appeared in the Levant cable, laid by Mr. Newall, who speaks of the destruction of the hemp by a species of "teredo." Mr. Siemens speaks to the same effect, and says, "This cable, which was

laid in 1858, and taken up again last summer (1859), was found to be beset by another enemy, in the shape of millions of small shell-fish or snails, accompanied by small worms, which had completely destroyed the unsheathed hemp, and eaten some circular holes in the gutta-percha." Professor Huxley wrote, as the result of his examination of these shells: "The specimens you sent me remove all doubt as to the nature of the mischief-maker in the cable. It is a bivalve shell-fish, the xylophaga, closely allied to the ship-worm (teredo), but distinguished from it, among other peculiarities, by not lining its burrow with shelly matter. The xylophaga turns beautifully cylindrical burrows, always against the grain, in wood; and I have no doubt it perforated the hempen coating of the cable in the same way. On meeting the gutta-percha it seemed not to have liked it, and to have turned aside, thus giving rise to the elongated grooves which we see. Nothing is known, so far as I am aware, of the range in depth of the xylophaga, so that I cannot answer your inquiry as to whether it is probable that cables immersed in 600 to 2,000 fathoms of water would be attacked or not."

In 1860 several portions of cable covered with hemp and steel wire were picked up in the Mediterranean off Minorca; these were found in places, and up to deep water, very much attacked by xylophaga, the hemp between the steel wires being eaten away into holes, with the regularity and spacing of those in a cribbage-board. As in previous cases, the gutta-percha was penetrated to various depths, but not more than the size of the shell-fish. It was generally considered that the xylophaga did not penetrate, owing to its dislike to gutta-percha; but some persons at the time thought there was a great deal of doubt about the point, for there was no sign along the great length of cable so damaged of any dislike, the main sign being that there had been *no time* for further penetration.

Subsequently some specimens were forwarded from Norway, giving the usual marks of a borer.

These marks have been noticed in many of the cables in the English Channel; the French Atlantic cable off Brest, and the Anglo-American off Valentia, bear similar marks.

In the repairs lately executed to the Key West and Punta Raca cable the teredo was again visible, and it may be assumed that various kinds of borers may be met with all over the world.

There are apparently different kinds of borers, for the results appear very different. In the hemp-covered cables of the Mediterranean the borer appeared of the nature of a bivalve shell, with an auger-mouth boring straight into the core. In the cables round the United Kingdom, as well as those on the Norwegian coast and the Atlantic, the borer appears to be more like the "teredo;" it makes a small hole, and then scores along the gutta-percha. No penetration beyond the surface is noticed, and it would appear that this worm enjoyed the hemp, occasionally trying the gutta-percha, which does not seem to its taste.

The specimens from the Persian Gulf are again of a different form, and show a most vigorous enemy, for the penetration of the gutta-percha to the conductor is a matter of short time, whilst the hole made is perfectly circular and very large.

Round the English coast there is a small borer, which, where it can get at the cable, penetrates direct to the conductor, and produces a fault. In the cables across the Irish sea to Wexford and Dublin these "borers" have been met, but on the Welsh side.

During some repairs to the Holyhead-Dublin cable it was noticed that "at about six miles from Port Crugmor (the landing-place) at every broken wire and open place in the sheath all the inner hemp serving is completely eaten away by worms, leaving the percha core exposed, which in one or two places is scored from the same cause."

At other points the gutta-percha was found pierced directly inwards, and a small worm found in each hole. At the same spot the hemp was found eaten away, and two or three different varieties of worms were noticed. Fortunately, some specimens of these were obtained, and preserved in spirit. When they were brought to London, Mr. Culley, of the General Post Office, forwarded some specimens to Dr. Carpenter, F.R.S., a gentleman well known for his investigation into marine life, for examination and report. With Mr. Culley's permission I am enabled to place this report before you.

University of London, Burlington House, W.
January 26th, 1875.

Sir,—I exceedingly regret the delay which has occurred in my reply to your communication of the 27th November last, with reference to the marine animals by which the telegraph cables are attacked.

Had I relied on my own judgment alone, I should have been able to answer you at once, as I thought that I recognised all your specimens as types with which my marine researches have made me familiar.

But in a matter of such importance I judged it better to obtain a corroboration or correction of my own judgment from the naturalists who rank as the highest authorities in this country on (1) marine worms and (2) crustaceans.

Dr. Mackintosh, who is now bringing out a complete work on the marine worms of Great Britain, and who is extremely conversant with their habits as well as with their form and structure, recognises three types, *lepidonotus equamatus*, *evarne imfar*, and *nereis pelagica*, all well-known British forms, and says, "I agree with you in acquitting them of all share in making the perforations in the coverings of the cable. They had only been lurking (after their wont) in the holes made by other forms."

The Rev. W. M. Norman (whose letter I only received this morning) agrees with me in identifying the minute crustacean as the *limnoria lignorum* of Rathké, known to British naturalists as the *limnoria terebrans*. This is a most destructive creature, whose ravages have long been a source of great injury to the woodwork of piers, bridges, harbour works, &c., often erroneously attributed to the boring of the teredo. A full description of its structure and habits was given by Dr. Coldstream in the "Edinburgh New Philosophical Journal," vol. xvi. p. 316.

Clearly, therefore, it is the *limnoria* that does the mischief to your cables. As its ravages were long ago noticed at Dublin it must be an old inhabitant of the Irish Sea. It is so small a creature that it would easily make its way through any fissure left by the separation of the wires of the iron sheathing; and it would

seem to me that the overlapping copper riband of the Messrs. Siemens would afford a surer protection.

Trusting that this report will be satisfactory to you,

I remain,

Your obedient servant,

(Signed) WILLIAM B. CARPENTER.

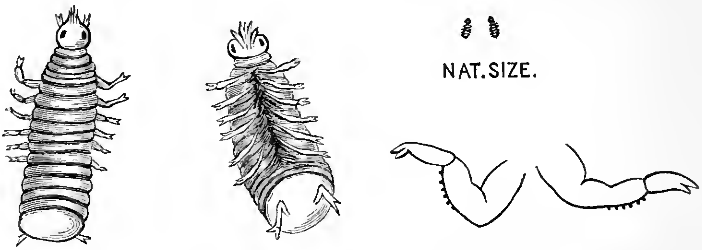
R. S. Culley, Esq.

I cannot, however, help differing from the opinion expressed relative to the other worms. It is true they do not penetrate the gutta-percha, but they unquestionably penetrate the cable and destroy the hemp. The holes made by the *limnoria* are small and well defined, and can be of no assistance to their larger brethren.

A short description and sketch of the *limnoria* is given by Dr. John Hall Gladstone, F.R.S., in *The Argonaut*, which runs as follows:—

The Electric Telegraph cable laid across the Irish Channel was found to be working badly, so a portion of it which was suspected was lifted up from the bottom of the sea ; it was found that the gutta-percha covering of the wires had been perforated by many holes, such as we are accustomed to see in old timber. Now the object of the gutta-percha, as is generally known, is to insulate the copper wire ; and, if at any point connection should be made between the metal and so good a conductor as the water of the sea, there is no chance of the message reaching the opposite shore. I believe none of the holes had actually penetrated to the wire itself, but they came dangerously near it. Two small marine creatures were found in these holes, and so suspicion rests upon them as having done the mischief. They have been properly preserved on microscopic slides by Mr. G. E. Preece, of the Engineers' Department of the General Post Office. They are about one-sixth of an inch long, and when magnified are seen to be small crustaceans, of which drawings are annexed. The head is furnished with at least five pair of claws and other appendages, and legs resembling those of the lobster are attached to the six first rings and the last ring of the body. I am told on good authority that it is a small creature well known in shallow seas, and named *limnoria terebrans*. If this borer should acquire a strong taste for gutta-percha, we can imagine what terrible results may be caused by the creature. One break-

fast that he might make may be more costly than that of the most luxurious Roman epicure in classic times; for he may destroy in a minute what will take thousands of pounds and a whole year to repair.



Upper side. Under side. Legs more highly magnified.

There are other "cable-borers," which are most destructive to land cables in various parts abroad, where the matter has become of a serious nature, as it is almost impossible to get wire to last in some places.

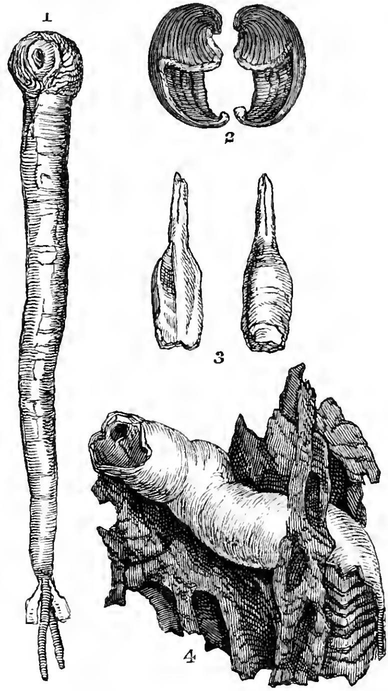
Of the various forms of marine "borers," the teredo or ordinary worm appears only to touch the core and not penetrate, whilst the bivalve and the *limnoria* penetrate and do actual injury, the remaining forms, equally with the *teredo navalis*, doing an immense amount of damage to the hemp-serving wherever they can find an entrance between the wires of the sheathing. The slightest opening appears to be detected at once, and the rapid destruction of the hemp immediately follows.

It is to be hoped that the introduction of this subject, one so interesting to the Submarine Cable Engineer, will bring forth further information from the members of the Society relative to those forms of animal life which are destructive to the durability of Submarine Cables.

I append an interesting description of the *teredo navalis* extracted from *The Colonies.*

Teredo is the name given by Linneus to a genus of testaceous or acephalous molluscs belonging to the family of the *pholadidæ.* There are about fourteen recent species recorded by naturalists, of which three are inhabitants of the British seas; and twenty-four others occur in a fossil state in various parts of the globe. Although the teredo is included in the same class of shelly animals as the oyster, the mussel, and the pecton,

a casual observer would, at first sight, trace but little resemblance between the two forms. They are, however, closely allied anatomically. The teredo, like all the members of the *pholas* family, is a borer; and the ravages it commits upon submerged timber have been the cause of drawing towards it a larger amount of interest than so obscure an animal would otherwise obtain. Let our reader picture to himself a kind of worm of a greyish colour, sometimes a foot in length, and six or eight lines in diameter, one extremity of which terminates in a sort of rounded protuberance, whilst the other is like a bifurcated tail (fig. 1). Such is the aspect of the teredo when removed from its tube. The rounded portion is inclosed by two small shelly valves (fig. 2), which protect only a trifling portion of the body properly so called. The liver and the ovaries lie in contact with one another, very far back, and behind the shell, whilst the branchiæ are placed in the posterior part of the body. The mantle, forming a sort of fleshy case, envelopes all the viscera, and afterwards divides into two tubes or siphons at the extremity, which the animal extends or contracts at will. One of these tubes serves to introduce the aerated water which bathes the branchiæ, and carries to the mouth the organic molecules which are necessary for the nutrition of the animal, whilst the other throws off the exhausted water, which collects in its passage the residue of digestion. Attached to these tubes are the two flattened siphonal pallets. These, when brought together, shut up the orifice, and inclose the two contracted tubes within it. After having attained their adult state, the teredos live a secluded life in the galleries they construct for themselves within the submerged timber (fig. 4) on which they first commence their attacks when leaving



1. "Teredo Norvegica" removed from its burrow. 2. Valves of do. 3. Siphonal shelly pallets of do. 4. Portion of tube showing internal concamerated structure.

the branchiæ of the mother. These galleries they line with a deposit of calcareous matter which forms the tube or protecting shell of the animal; each animal being independent of its neighbour, their burrows having no communication with one another. This circumstance at one time caused the teredo to be classed amongst those creatures which have the sexes combined in the same individual; but such is now discovered not to be the case, and, notwithstanding the cenobite life of the otherwise gregarious "shipworms," they consist of two distinct sexes. At a certain period the females emit their eggs, which are arrested within the folds of the respiratory organs. In this singular nest the eggs are hatched, and remain for a short time under a different form to that which they subsequently acquire. At their last metamorphosis they become free, and float about in the water in search of the first piece of wood that presents itself, to commence life on their own account.

It has been suggested that the common species, *teredo navalis*, which now unfortunately swarms in our Northern seas, was originally introduced from a warmer region. However that may be, the ravages of this apparently insignificant mollusc are terrible. When we examine its fragile shell, semi-transparent tissues, and soft body almost incapable of motion, we can hardly conceive that the teredo is a creature to be feared; and yet it is, in reality, one of the most formidable enemies of man. It attacks wood under water much in the same way as the indefatigable *termis*, or "white ant," attacks it on land; it does not content itself with a superficial station, but seeks a secure retreat within the log; and, where once these hidden destroyers take up their residence, they soon honeycomb, as it were, the solid oak, reducing the timber to a mere shell. In former times, before the practice of coppering the bottoms of ships became general, many a stately vessel fell a sacrifice to their silent ravages; and about the middle of the last century fears were entertained for the safety of the kingdom of Holland, one-half of which was nearly engulfed in the waves, in consequence of the piles which support her enormous dykes having all been undermined and riddled by the teredos. Mr. Thornton has put on record an interesting account of the rapidity with which these molluscs destroyed the woodwork in the harbour of Port Patrick; and, later on, the Teignmouth Bridge, in Devonshire, fell a sacrifice to their rapacity. On every coast where there is any submarine woodwork not effectually protected by artificial means from the attacks of the teredo, there will the busy creature be found plying its inevitable task, and

undoing, in its own silent and mysterious way, the industry of mankind. The rapidity of its growth, and the destructive swiftness with which it works, are scarcely credible. A piece of deal, after a submersion of only forty days in one of the royal dockyards, was, so Sir Everard Home relates, completely riddled by teredos, some of the creatures having attained a considerable size. In the short space of four years solid oak piles in the Plymouth dockyard, which had been under water, were found to be so perforated that they had to be replaced with new ones. In an interesting account of his observations on the anatomy of the teredo, Sir Everard Home throws some light on its method of boring into the hard timber in which it constructs its burrow. He tells us he noticed that a part of the animal which was inclosed between the two concave boring shells was exposed in the centre; and that from the middle of this exposed part projects a kind of proboscis, which, in the living animal, had a vermicular motion; its extremity being covered by a cuticle not unlike the cornea of the eye. On removing this the cavity immediately beneath it was found to contain a hard brown-coloured gelatinous substance. Sir Everard remarks, that, as this proboscis has no orifice, there is reason to believe that it adheres to the wood, acting as a centre-bit, while the animal at the same time is at work with the shell, and thus the canal in the wood is perfectly cylindrical. Woodward tells us the operations of the teredo first suggested to Brunel his method of tunnelling the bed of the Thames. To effectually preserve all submerged wooden structures from the depredations of the "shipworm" has long been the study and aim of the architect and engineer. Our ships are now rendered secure by being bottomed by a sheathing of copper or Muntz metal; but this method is not so applicable to structures erected in the sea, such as piers, jetties, and the like. The only certain way of preventing the attacks of the teredo upon submerged piles appears to be by covering all the parts which are continually beneath the surface of the water with short, broad-headed iron nails. The action of the sea-water on these nails produces a strong coating of rust, which is fatal to the young teredo, and is said to be even superior to a copper sheathing. That ingenious French naturalist, M. de Quatrefages, suggests a plan by which to get rid of these marine pests by destroying them in the egg; in fact, to "prevent their birth." To attain this object, he says it is sufficient to dissolve in the water which the mothers respire an infinitely small quantity of any salt of mercury, lead, or copper. He further remarks:—"To preserve the

submerged wood of our marine docks and wharfs it is therefore only necessary from time to time to throw a few handfuls of these salts into the surrounding water. Fecundation would thus be entirely arrested, and the eggs would perish before they were developed, and consequently the species would be completely exterminated in our harbours and docks in the course of two or three seasons." From experiments recently made, it would appear that timber long macerated in a solution of corrosive sublimate is proof against the perforations of the teredo.—*The Colonies.*

The CHAIRMAN: The subject with which these two papers deal is not only very interesting, but very important, and, I must add, most serious, and any observations which will make us better acquainted with the dangers we have to deal with, and the particular method of guarding against these ravages, will be of great importance and interest.

Mr. ADAMS: I did not understand whether both the male and female insect bored; and it would be interesting to know whether the boring is done for food, or for the purpose of propagation. In the Black Sea a copper tape-covered cable, was, I believe, laid some years ago by the Indo-European Company on account of these marine insects, but whether the copper really preserved the cable I cannot say—perhaps some member can say if the insects there found were of the same description. I have heard that the Black Sea is well known as containing these insects.

Mr. WARREN: There is one source of mischief to cables from marine life which, I believe, is a novelty, at least I am not aware of any similar instance being recorded. During the laying of the cable between Para and Cayenne, the cable was buoyed about thirty-five miles from Cayenne, for the purpose of attaching a T-piece junction towards Demerara. When we proceeded the next day, after completing the section from Para to Cayenne, to form a junction to connect it with the Demerara cable, and picked up the buoyed end, we found the cable had gone down in insulation from a fault which, by tests, we localised to be about 126 miles from Para. We had to lift it for repairing, and within a length of thirty

miles we found no less than six faults, each of which contained fragments of a bony nature, which we supposed were caused by the bite of some marine animal, as the teeth were left in the cable. As these faults appeared after the cable was laid, we could not explain how a fish could attack a cable in this way.

We removed these faults and the cable was then as good as ever. We went out there again at the beginning of the present year, when we found the cable faulty again at the same place, or about 129 miles from Para, and here we found two faults similar to the former ones, and produced by the same means and in the same way. We subsequently had occasion to go to the same spot again in July, when we picked up three more faults within 150 miles of Para. In one fault there was found a very large piece of hard bony substance, about an inch and a-half in length, the part in the cable measuring something like an inch round. We were at a loss to account for these faults at first, or to identify the tooth or tusk-like instrument with that of any fish we had seen, but when we got to Demerara we had no hesitation in attributing it to the teeth of the saw-fish, which exists plentifully in the water along the more northern portions of the Brazilian coast. Photographs of these faults were taken, which I should like to have laid before the meeting. I think it is a curious fact, and one not without importance to Telegraph Engineers, for there is no doubt but that these faults are confined to a given portion of the cable. Since last August another serious fault was localised about 130 miles from Para. These fish are known to inhabit that portion of the water, and, do what we will to remove the faults, they are sure to occur again. A heavier clad cable, only a few miles nearer to Para, has never been found to be so attacked. When we have been working on this cable, we have found a description of black whale in the neighbourhood of these faults, which, we are informed, are objects of attack by these saw-fish ; and we were informed that the smaller kinds of these fish were so plentiful that at high spring-tides, when the sea washes over the embankments at Demerara, a large number of the fish are left by the receding tide in the trenches of the plantations.

The Secretary: One of the Eastern cables was injured in the

same way. I have seen specimens of the Mediterranean cable bitten by sharks, with the teeth remaining in.

Mr. WARREN: We supposed at first that these faults were occasioned by the bites of sharks, having fallen in with several of them, but the teeth of that fish are close together, so that it was impossible to get the teeth between the ironwork of the cable. In this case the teeth were left in the cable between the iron casing and the core. The greatest depth of water at which we found faults of this kind was seventy fathoms, but the average depth was about forty.

Mr. WILLOUGHBY SMITH: With reference to these boring insects, I think the cause may be traced to the fact, that in the early cables the serving was saturated with a mixture of tar and tallow, and probably that may have been the attraction. A few years ago the core of the cable laid from Dover to Calais was found to have been injured by a small insect resembling a white maggot, with two very prominent black eyes. I believe that is the only instance in which that cable has been so injured. Where those insects were found, the iron wires had been destroyed by age, and the serving becoming exposed they had attacked it vigorously, whilst some more venturesome than the others had bored into the gutta-percha, and, in two instances, had reached the copper wire. The experimental line laid in 1850 had no serving or other covering over the gutta-percha, and in no instance in the many lengths of that line which have been recovered at various times have there been any signs of the gutta-percha having been attacked by insects.

The CHAIRMAN: It appears to me that there is considerable probability of the correctness of the remark of Mr. Willoughby Smith, that the casing of cables with hemp and tar attracts the *limnoria* and other destructive insects which appear to be very prolific in the Irish Sea. These insects appear to have a liking for gutta-percha, and they bore straight to the conductor, as is seen from the specimens sent by Mr. Mance. India-rubber, on the other hand, certainly appears to be free from that species of attack, while the gutta-percha specimens show that the insulating medium has been injured to an extent to give rise to serious mischief, and

that great attention is requisite for the preservation of gutta-percha cores against these attacks by proper armouring. I have now to ask you to return your thanks to Mr. Mance and Mr. Preece for these very interesting communications.

The vote of thanks to Mr. Mance and Mr. Preece was unanimously passed.

The following Candidates were balloted for and declared duly elected:—

As Foreign Members:

Dr. F. Dehms.
F. C. C. Nielsen.
C. C. Sonne.

As Members:

G. Bird.
Capt. F. W. Heneage, R.E.
Lieut. H. P. Nicholles, R.E.
Lieut. E. F. Rhodes, R.E.
Lieut. H. Rawson, R.E.
Sir David Salomons, Bart.
W. J. Wilson.

As Associates:

F. Jacob.
E. Applegarth.
A. Peters.
W. D. Smallpiece.
W. F. Nosworthy.

The Meeting then adjourned.

The Fortieth Ordinary and Fourth Annual General Meeting was held on Wednesday, the 8th December, 1875, Mr. Latimer Clark, President, in the Chair.

The President announced that the Ballot for President and Members of Council and Officers of the Society for the ensuing year would take place, and be closed at half-past eight o'clock.

Messrs. A. Bell and R. von Fischer Treuenfeld were appointed Scrutineers.

The Secretary then read the Annual Report from the Council as follows:—

ANNUAL REPORT FOR THE YEAR 1875.

As the present is the Annual General Meeting appointed by the Society's Rules to receive the Annual Report, your President and Council have now the pleasure of informing you of the operation of the Society during the past year, and of its present state.

The same kindness and privilege shown to us by the Institution of Civil Engineers during the three previous years of our meetings has been extended in an equally kind and handsome manner throughout the present year, and the thanks of the Society are again largely due to the President and Council of the Institution for their continued good wishes and help. After the previous Annual General Meeting the thanks of this Society were communicated to the President and Council of the Institution, and Members will be glad to know that the parent Institute is gratified at the rapid increase of the Society.

The numerical progress of the Society still continues in a very satisfactory manner, the following being the actual additions during the past year, inclusive of those gentlemen to be balloted for to-night.

	1875.	1874.
Foreign Members - - -	41	24
Members - - - - -	25	17
Associates - - - - -	47	76
Students - - - - -	2	8
	115	126

From these figures it will be noticed that there has been a large increase in the number of Foreign Members during the past year as compared with the previous, showing that their acquaintance with the Society and its objects is extending. The additional number of Members compared with 1874 shows an increase, whilst a decrease is visible amongst the Associates, showing how widely the Society has spread, and that necessarily there must be fewer left to come within its scope.

NUMBERS ELECTED IN 1875.

1874.		To Dec. 8.	Dec. 8.	Total.
4	- Hon. Members - - -	0	0	0
81	- Foreign Members - -	38	3	41
202	- Members - - - -	24	1	25
346	- Associates - - - -	39	8	47
15	- Students - - - - -	2	0	2
648		103	12	115

At the present time the total number of Members of all classes belonging to the Society amounts to 763, consisting of the following classes :—

Hon. Members - - -	4
Foreign Members - - -	122
Members - - - -	227
Associates - - - -	393
Students - - - -	17
Total - - -	763

Against 648 of 1874.

The following statement gives a comparison of the numbers during the preceding years, 1874, 1873, 1872; the time being at the Annual General Meeting or the end of the year:—

LIST OF MEMBERS, &c.

	1872.	1873.	1874.	1875.	1875.
Honorary Members .	0	3	4	4	4
Foreign „ .	25	57	81	119	122
Members . .	155	185	202	226	227
Associates . .	170	270	346	385	393
Students . . .	2	7	15	17	17
Totals . .	352	522	648	751	763
Increase .		170	126 296	103 399	115 411

During the year the Council regret to announce that the Society has lost by death two of its valued members. Amongst these are Mr. Charles Becker, who died early in the year, well-known to all, and one who was highly esteemed for his personal character and his great scientific knowledge.

More lately still we have lost from our list the wide-world name of Sir Charles Wheatstone, to whom telegraphy and electricity owe so much. The President has lately given you an interesting sketch of his life and works, and the Council can now only allude to the fact of his death with the greatest regret, and grieve that he should have been cut off at a time when his mental faculties were, in spite of his advanced age, in most active operation, giving constantly to the world the benefit of new improvements. A vote of condolence and sympathy was duly passed to the family—which has been since acknowledged.

There has been one name added to the list of Honorary Secretaries during the year, that of Mr. John Aylmer, a Civil Engineer, resident in Paris, who has been elected as the Honorary Secretary for France. The following is now the list of Local Honorary Secretaries:—

LOCAL HONORARY SECRETARIES.

JOHN AYLMER, 4, Rue de Naples	FRANCE.
W. E. AYRTON, Professor of Natural Philosophy, Imperial College, Yokohama, Japan	JAPAN.
CHARLES BURTON, Telegraph Engineer, Buenos Ayres	ARGENTINE REPUBLIC.
J. M. COLLETTE, Engineer of the Netherlands Telegraphs	NETHERLANDS.
Le Commandeur E. d'AMICO, Director-General of the Italian Telegraphs, Rome	ITALY.
FRÉDÉRIC DELARGE, Engineer of the Belgian Telegraphs, Brussels	BELGIUM.
C. L. MADSEN, Great Northern Telegraph Company, Copenhagen	DENMARK.
C. NIELSEN, Director-General of the Norwegian Telegraphs, Christiania	NORWAY.
Colonel D. ROBINSON, R.E., Director-General of India Telegraphs, Calcutta	INDIA
Don RAMON PIAS Director-General of the Chilian Telegraphs, Santiago	CHILI.

The following gives a *resumé* of the various meetings held during the year:—

JANUARY 13.—President's Inaugural Address.

,, 27.—"The Heliograph of Mr. Henry Mance," by CHARLES BECKER.

"On Vibrations due to Earth-Plates," by JAMES GRAVES.

FEBRUARY 24.—"On Induction between Suspended Wires as affecting Automatic Transmission," by R. S. CULLEY.

MARCH 10.—"On the Imperfect Contacts which occur in Signaling with rigid Contact Points," by THOMAS A. EDISON of New York.

"Batteries and their employment in Telegraphy," by JAMES SIVEWRIGHT.

MARCH 24.—Ditto, Ditto.

APRIL 14.—"Batteries," by ALFRED BENNETT. Discussion on above and Mr. Sivewright's Paper.

APRIL 28.—Adjourned Discussion on above.

MAY 12.—"Battery Resistance Measurement," by FREDERICK HAWKINS.

"Earth Connections for Lightning Conductors," by Lieut.-Col. STOTHERD, R.E.

NOVEMBER 10.—"On the respective Merits and Durability of Gutta-Percha and India-Rubber Joints," by HENRY MANCE.

NOVEMBER 24.—Adjourned Discussion on above.

"Experiments conducted for the purpose of ascertaining whether the Teredo prefers Gutta-percha to India-rubber," by HENRY MANCE.

"On Cable-Borers," by G. E. PREECE.

DECEMBER 8.—Annual General Meeting.

"On the London Street Work," by CHARLES FLEETWOOD.

The Annual Soirée of the President will be held on Tuesday, the 21st December, at Willis's Rooms, St. James's, when it is hoped a display of scientific instruments of novelty and interest will be exhibited.

In addition to the above-mentioned Papers the Journal contains a series of Original Communications, and also numerous valuable Abstracts and Extracts.

It has been considered desirable by the Council to bring before the Members a slight alteration in one of the existing rules.

Rule 12 enacts that ballots shall take place at each of the Society's meetings; it is proposed that the ballot shall take place only at the first meeting in each month; it is believed that this alteration will be beneficial. It is a practice carried out by almost every Institution.

A resolution will be brought forward at the first meeting in January to make the proposed alteration a part of the Rule.

The following notices were issued to Members before the commencement of the present Session relative to Papers and Communications and also as to the addition of Members:—

SOCIETY OF TELEGRAPH ENGINEERS,
4, BROAD SANCTUARY, LONDON.

THE Secretary is desired to bring before the notice of Members and Associates that the Society's usual Meetings will be resumed early in November, and that these Meetings are not held fortnightly, but on the second and fourth Wednesday in the month, due notice being given of the exact days of meeting.

It is desirable that Members and Associates who are willing to contribute papers to be read during the ensuing Meetings should forward them to the Secretary at an early date, in order that they may be approved by the Council, and, when deemed necessary, printed for distribution, in order to invite discussion.

The Secretary is also desired to call the attention of Members and Associates to the subject of "Papers" and "Communications," and to invite them to forward articles of both classes, in order that the interest of the Society and its Members may be maintained. "Papers" are deemed to be Original Communications of such interest as are worthy to be read before the Meetings, in order that the special points referred to in the Paper may be brought before the Members and Associates and discussed, so that the progress of Telegraphy may be advanced. These Papers, with their discussions, will be subsequently published in the Journal.

"Communications" may be termed Papers on various new and interesting points which frequently arise in the every-day working of the Telegraph, and which it is not only interesting but important that the general body of Members should know. Various forms of Communications may be seen in the Journal. And the attention of Members is specially invited to the forwarding of such Communications, for it may be safely relied upon that the frequent publication—whether before the Meetings or in the Journal—of points new to Members which have been elucidated, will be of most material value to all Telegraph Engineers and to the progress of Telegraphy.

Such Communications will be read before the Meetings as may be deemed desirable, and subsequently printed in the Journal.

A list of subjects on which it is desirable that Papers should be contributed is attached.

The Journal of the Society, containing the Proceedings, Original Communications, and Abstracts of Electrical and Telegraphic interest, is now published quarterly.

GEO. E. PREECE,

Secretary.

November 2nd, 1875.

SUBJECTS FOR PAPERS AND COMMUNICATIONS.

On the Action of Radiation on the Electric Conductivity of Selenium.

On Lightning and Lightning Protectors.

On Duplex Telegraphy.

On Duplex Telegraphy as applied to Submarine Cables.

On Quadruplex Telegraphy.

On the Action of Marine Life on Submarine Cables.

On Surveying and Dredging to find Proper Routes for Submarine Cables.

On the Durability of Submarine Cables.

On Machinery for the Submersion and Recovery of Submarine Cables.

On Cables for Rivers, Canals, &c.

On the Localization of very Minute Faults in considerable lengths of Submarine Cables.

On the Manufacture of Iron Wire for Telegraph Purposes.

On the Preservation of Iron Wire in the vicinity of Towns and Manufactories.

On the Gauge of Wire for Long Circuits, and the Advantage of using Wire of a higher Conducting Material than Iron.

On the Manufacture of Gutta-percha for Insulating Purposes.

On the Manufacture of India-rubber for Insulating Purposes.

On the Change of Insulation of Material, whether Gutta-percha or India-rubber, with Change of Temperature and Climate.

On the Residual Charge or Polarization of Insulating Materials.

On Electrometers, especially with regard to a Cheap Form of Electrometer.

On the best System of laying Underground Wires.

On the Maintenance of Underground Telegraphs.

On Aerial Cables and their Insulation.

On making Joints in Underground Insulated Wires, where a large Number have to be dealt with in the Open Air.

On the Effect of Residual Magnetism, and of "extra" Currents on Automatic Telegraphy.

On Automatic and Fast-speed Telegraphy.

On the Relative Degree of Accuracy to be expected from Sound and Sight-reading (Morse), and on the value of the Morse Ribbon as a record in Cases of Error.

On Earth Currents, and the best mode of observing them.

On Earth Plates in dry places and rocky localities.

On Experience and Experiments bearing on the relative Constancy and Working Properties of different Batteries.

On the Theory of the Galvanic Battery; with a Critical Review of the Evidence for and against the "Contact" and "Chemical" theories.

On Electric Torpedoes.

On Electric Exploders.

On Induction Machines for the Production of Light.

On Insulators of extreme perfection for Indoor Work and for Laboratory Experiments.

On Induction in Overground Wires.

On Line Insulators.

On the Use of Living Trees as Supports for Telegraph Wire.

On Geometrical Illustrations of Electrical Laws.

On Field and Military Telegraphy.

On Pneumatic Tubes as a mode of transmitting Telegrams.
On Condensers of a Permanent Character.
On Electricity as an Aid to Meteorology.
On Electric Signaling on Railways and in Trains in motion.
On Electro-Metallurgy.
On Thermo-Electricity.

THE SOCIETY OF TELEGRAPH ENGINEERS,
4, BROAD SANCTUARY, LONDON,
November 2nd, 1875.

SIR,—I beg to inform you that the first meeting of the Society for the ensuing Session will be held on Wednesday the 10th November, at 8·0 p.m., at the Institution of Civil Engineers, when a paper on "The Durability of Joints in India-rubber and Gutta-percha Cables," by Mr. Henry Mance, will be read. The subsequent meetings will be held on the following dates:—

November 24th and December 8th.

1876.—January 12th and 26th. February 9th and 23rd. March 8th and 22nd. April 12th and 26th. May 10th.

A card of these Meetings is inclosed for your future reference.

At these Meetings, according to Rule 48,—

"Every Member and Associate shall have the privilege of introducing one stranger, to be present at the Ordinary General Meetings of the Society, on writing his name in a book provided for that purpose, or sending with him a card signed with his name, according to a form provided."

These forms may be obtained by application to the Secretary.

I would respectfully draw the attention of the Members and Associates of the Society to the "objects" of the Society.

"The SOCIETY OF TELEGRAPH ENGINEERS is established for the general advancement of Electrical and Telegraphic Science, and more particularly for facilitating the exchange of information and ideas among its Members."

These objects may be fulfilled, firstly, by increasing the numbers of the Society, and, secondly, by a greater exchange of information and ideas among its Members.

The following extracts from the Rules will draw your attention to the qualification of Candidates; and I beg to inclose some forms in the event of your meeting with gentlemen desirous of joining a Society whose spread has been so rapid, and whose numbers now approach 700.

"MEMBERS AND OFFICERS.

"1. THE SOCIETY OF TELEGRAPH ENGINEERS shall consist of Members, Associates, Students, Foreign Members, and Honorary Members.

"2. MEMBERS.—Every Member shall come within one of the following conditions:—

"(*a.*) He shall have been regularly educated as a Telegraph Engineer (according to the usual routine of pupilage), and have had subsequent employment for at least five years in responsible situations;

"(*b.*) Or he shall have practised on his own account in the profession of a Telegraph Engineer for at least two years, and have acquired a degree of eminence in the same;

"(*c.*) Or he shall be so intimately associated with the science of Electricity or the progress of Telegraphy that the Council consider his admission to Membership would conduce to the interests of the Society.

"3. ASSOCIATES shall be persons of more than twenty-one years of age, whose pursuits constitute branches of Electrical Engineering, but who are not necessarily Telegraph Engineers by profession, also persons who are so intimately associated with the science of Electricity or the progress of Telegraphy that the Council consider their admission as Associates would conduce to the interests of the Society.

"4. STUDENTS shall be persons not under eighteen and not over twenty-one years of age, who are serving pupilage to a Telegraph Engineer, or who are studying Natural Science or Telegraphy, and are duly recommended by a Member.

"5. FOREIGN MEMBERS shall be *foreigners* residing abroad, engaged in, or intimately connected with, the practice of Telegraph Engineering or Electrical Science, whose admission to the privileges of membership the Council may consider would conduce to the interests of the Society."

The following extracts are with reference to the amount of fees and subscription :—

"24. Every Member shall contribute the sum of Two Guineas, every Associate the sum of One Guinea, every Foreign Member One Pound, and every Student Half a Guinea, annually to the Society. Honorary Members shall not be required to pay any contributions. Any Member or Associate residing abroad, or absent for nine months in the year, shall contribute the sum of One Pound annually.

"25. The annual contributions shall be payable in advance on the 1st of January in each year.

"28. Any Member may compound for his annual subscription by the payment, in one sum, of Twenty-one Pounds. An Associate or Foreign Member may also compound for his subscription by the payment of Ten Pounds."

I also inclose a form for transfer from the class of Associate to that of Member, as it has frequently been noticed that there are gentlemen whose experience, qualifications, and knowledge fully entitle them to the higher rank of "Member" of the Society.

I would particularly draw your attention to the accompanying Circular relative to "Papers" and Communications," in the hope that during the ensuing Session the Council may receive from you some communication which may tend to the "General advancement of Electrical and Tele graphic Science."

I remain, your obedient Servant,

GEO. E. PREECE,

Secretary.

THE RONALDS LIBRARY.

This Library was, during the year, removed from Battle, the residence of the late Sir Francis Ronalds, to the Society's rooms at 4, Broad Sanctuary, where shelves and accommodation were at once found, and the books, numbering about 6,000 volumes, placed on the shelves. The books are not yet arranged in proper order, as it will be necessary first to complete the Catalogue. This Catalogue contains as many as 13,000 entries, and its preparation for the

printer is a work of some difficulty. The first sheet is now nearly complete, and will be shortly forwarded to Members with a request to establish a library fund. It is hoped that the books will be soon arranged in their proper places ready for reference at any time.

The Council congratulates the Members on their excessive good fortune in the acquisition of so unique and valuable a library, and also on the continued success and progress of the Society. From the accompanying statement of the financial position of the Society (pp. 388, 389), it will, however, be seen that the amount of unpaid subscriptions is abnormally large. They would, therefore, strongly urge upon those Members and Associates whose subscriptions are still outstanding the necessity of remitting them as early as possible to the Honorary Treasurer.

STATEMENT OF RECEIPTS

By the Treasurer, for the

RECEIPTS.

		£ s. d.	£ s. d.
By Balance 31st Decr. 1874			62 4 10
,, Subscriptions	1873	8 6 0	
,, ,,	1874	64 12 0	
,, ,,	1875	413 7 0	
,, ,,	1876	13 16 6	
,, ,,	1877	1 0 0	
			501 1 6
,, Publishing Fund 1874		3 2 6	
,, ,, 1875		15 17 6	
			19 0 0
,, Composition of Life Subscriptions			52 0 0
,, Sale of Journals			13 8 2
,, Gain on Exchange			0 1 8
		Total	£647 16 2

STATEMENT OF

On 31st

RECEIPTS.

	£ s. d.
By Donations	200 0 0
,, Compositions	161 15 0
Total	£361 15 0

ESTIMATE OF ASSETS

On the 31st

ASSETS.

	£ s. d.
To Unpaid Subscriptions	766 7 0
,, Furniture (half original cost)	142 18 7
,, Journals sold or in hand	442 9 10
Cash Balance	162 11 1
	£1,514 6 6

AND EXPENDITURE

year ending 31*st December*, 1875.

EXPENDITURE.

						£	s.	d.
To Salaries—Secretary	145	0	0					
Secretary's Clerk	60	0	0					
Treasurer's Clerk	10	0	0					
						215	0	0
„ Petty Expenses, including Postage—								
Secretary	50	0	0					
Treasurer	2	3	5					
						52	3	5
„ Stationery and Printing						20	13	9
„ Shorthand Reporter						22	11	6
„ Attendance and Refreshments at Meetings						20	15	5
„ Furniture						28	0	0
„ Rent and Taxes						100	0	0
„ Overpaid Subscriptions refunded						1	1	0
„ Expenses removal of Sir F. Ronald's Library						19	14	3
„ Insurance of Library						5	5	9
Balance Cr. ...						162	11	1
Total						£647	16	2

(Signed) C. E. WEBBER, Major R.E., *Treasurer.*

We have compared the above Account with the Vouchers and Cash Books, and find it to be correct, leaving in the hands of the Treasurer one hundred and sixty-two pounds eleven shillings and a penny.

J. WAGSTAFF BLUNDELL, } *Auditors.*
FRED. CHAS. DANVERS, }

CAPITAL ACCOUNT

December, 1875.

EXPENDITURE.

	£	s.	d.
To Furniture	285	17	3
Balance Cr.	75	17	9
	£361	15	0

AND LIABILITIES

December, 1875.

LIABILITIES.

	£	s	d.
By Subscriptions in advance	21	16	6
„ Outstanding Accounts—Salaries	7	0	0
Furniture	4	9	3
Rent, Fuel, and Taxes	143	14	6
Printing and Stationery	721	0	9
Balance Cr. ...	616	5	6
	£1,514	6	6

The following paper was then read :—

UNDERGROUND TELEGRAPHS—THE LONDON STREET WORK.

By Charles Fleetwood, Postal Telegraph Department.

The underground telegraph system of the metropolis has now become so great, entailing so large a staff to attend to it, and embracing so extensive a mileage of wire, that some description of it may prove interesting.

From the original *five* wires used by Cooke and Wheatstone, in July 1837, between Euston and Camden, to test the success of the original five-needle telegraph, the system has gone on increasing, latterly with rapid strides, culminating at the present time into 750 different wires alone entering the central station, and a total mileage of over 3,500 miles of gutta-percha-covered wire.

The first underground telegraph—a specimen of which is now shown—consisted of wood cut with sloping sides A-shaped, and tarred; grooves were cut at the sides and at the top, and the five copper wires inserted, each wire being previously insulated with cotton soaked in resin.

On the successful issue of the Cooke and Wheatstone telegraph, it was started commercially, and a line of five wires was placed underground in lead pipes between Paddington and Drayton; this line became defective, and in 1841 was replaced by posts and overhead wires. The existing lines created by Mr. Cooke from this period were in 1846, on the incorporation of the Electric Telegraph Company, led by wires in iron pipes to the first office in London, 345, Strand. In the following year the system was extended under the streets to the new central station at Founders' Court, Lothbury, which office was opened on the 1st of January, 1848; the total system of the Company at that time reached 1,500 miles of telegraph wire erected and in progress—a mileage less than one-half the present system under the streets of London alone.

The system adopted at that time appears to have been the right one, and thoroughly suited to the many requirements of telegraphy

under streets, for up to the present time but few variations have been made.

The wires were formed into cables and drawn through iron pipes which had been carefully laid; the wires were of No. 4 gauge, a solid No. 16 copper wire of the weight of 62lbs. covered with 86 lbs. gutta-percha. These cables were connected to what were termed "testing-posts;" these were hollow posts (with a door) placed above the pipes, and standing up like a street post—doubtless many present remember them well; into them the wires were led. The wires were connected in these boxes by a mechanical joint, which it was easy to open and disconnect for testing.

The changes which have taken place are the introduction of a smaller-sized wire, the abandonment of the gutta-percha mechanical joint, and the substitution of flush-boxes for the old "testing-posts."

One extraordinary form of accident developed itself with the "post;" gas escaping from the pipes found its way through the telegraph pipes into the "post." The ignition of a match against the side of the "post" by a passing smoker was frequently the cause of an explosion and the bursting the door of the "post," and as a rule many interruptions of communication.

The street-work of London consists of about 3,500 miles of No. 7 gauge gutta-percha-covered wire (copper wire, No. 18 gauge = 39 lbs. per statute mile, gutta-percha to No. 7 gauge = 46 lbs. per statute mile, diameter ·171), wrapped with tape and tarred.

The wires are drawn into cast-iron pipes of 3 in. and in some cases 4 in., diameter.

This system connects the central office, St. Martin's-le-Grand, with the several provincial railways and the main road lines of telegraphs; it also serves a large number of the metropolitan telegraph offices.

The total length of pipes is 110 miles. There are five main trunk lines radiating from the central station, as follows:—

First, *viâ* Newgate Street, Holborn Viaduct, Oxford Street, Western District Post Office, Bayswater Road, Notting Hill, Uxbridge Road, Turnham Green, Brentford and Hounslow, where they meet the overground wires in the Bristol Road.

In this route, from the vaults at the New General Post Office under the footpaths in Newgate Street to the corner of Giltspur Street, there are one 3-inch and two 4-inch cast-iron pipes, containing in all 240 wires; of these thirty are carried into the Hatton Garden Post Office, where they are connected to the over-house system.

At the corner of Gray's Inn Road, Holborn, there is a branch line of 3-inch pipes and 70 wires, serving the Great Northern Railway, King's Cross, the North-Western District Office, Euston Station, and passing up Eversholt Street, Camden Road, Adelaide Road, to the north end of Primrose Hill tunnel, on to the London and North-Western Railway.

Another branch line leaves this route at Victoria Gate, Bayswater Road. There are 70 wires, 30 of which join the Great Western Railway at Paddington, where they pass through a test-box. The remaining 40, after passing through the same test-box, are continued underground to Harrow Road Bridge, and there connected to the overground wires, *viâ* the Grand Junction Canal.

Second route, from St. Martin's *viâ* Ivy Lane, St. Paul's Churchyard, Ludgate Hill, Fleet Street, Strand, Whitehall, Great George Street Westminster, Birdcage Walk, Buckingham Palace Road, Chelsea Suspension Bridge, Wandsworth Road, and Clapham Common, joining the overground system.

On this route there are many deviations; the first occurs at the corner of Wellington Street, Strand, where forty wires leave for the South Western Railway, Waterloo Station, by Waterloo Bridge; the next at Trafalgar Square, for the War Office, Piccadilly Circus, and the St. James's Street offices; and after passing down the Mall in St. James's Park it strikes off up Constitution Hill to Hyde Park Corner, thence to Knightsbridge, South Kensington, and Young Street, Kensington, Post Offices. From St. Martin's to Wellington Street, Strand, there are two cast-iron pipes, one of 4-inch and the other of 3-inch diameter, with 180 wires; beyond this point to the West Strand Office there are two 3-inch pipes, only one of which is continued on.

The third route passes by Cheapside, King William Street, over London Bridge to the South-Eastern and South Coast Railways,

continuing down the Borough to the South-Eastern District Post Office, through which the wires pass, and thence down Great Dover Road to New Cross, joining the overground wires running to Beachy Head at the point where the Brighton Railway crosses the road, and also the South-Eastern Railway at that Company's Station, New Cross.

At the corner of Princes Street, opposite the Mansion House, there is a junction, from which the wires pass through Cornhill, Leadenhall Street, Aldgate, to the Eastern District Post Office, and thence to the West India Docks.

From the corner of Commercial Road the system proceeds to Grove Road, and is connected with the Ipswich overground wires *viâ* Duckett's Canal. This route is now being extended to Maryland Point, Stratford.

Down Cheapside, as far as Princes Street, there is a 4-inch cast-iron pipe with 90 wires, as well as five 7-wire cables, making altogether 125 wires; beyond this point the pipes are of 3-inch diameter; 72 wires pass down King William Street and 70 along Cornhill.

The fourth route is *viâ* Gresham Street to the corner of Moorgate Street, Finsbury Square, City Road, Old Street, Kingsland Road, Stamford Hill, Bayley's Lane, on to the River Lea, where the wires are connected to the Cambridge Road line.

A branch line of pipes from the junction-box at the corner of Moorgate Street supplies the offices of the Anglo-American, Direct United States, and the Submarine Companies, and also the Stock Exchange.

At Telegraph Street, 26 wires join the overhouse system; at London Wall, 8 wires lead to Broad Street Station; at Providence Row, 50 wires pass down Worship Street round to the Great Eastern Railway Station, Bishopsgate Street, where they are connected to the Great Eastern and Continental system.

Between St. Martin's and Finsbury Square there are two 3 inch pipes and 130 wires, beyond this point one 3-inch pipe.

The fifth route leaves St. Martin's at the north end of the building in Angel Street, and thus by Aldersgate Street, Goswell Road, High Street Islington, passing through a test-box at the

Northern District Post Office, at the corner of Packington Street; thence by Cross Street, Upper Street Islington, Holloway Road, up Highgate Hill, meeting the overground wires at East End Finchley, Birmingham Road line. This route comprises a 3-inch pipe and 60 wires as far as the Northern District Office, 25 wires to the Railway Bridge, Holloway Road, and 20 beyond.

In addition to these main routes there are several short connections, such as, from the Submarine Office Threadneedle Street, to London Bridge, *viâ* Birchin Lane, Victoria Station to Marble Arch, &c.

If the whole system, as now comprehended, was to be done *ab initio*, it is probable that a more advantageous series of main trunk lines would be adopted, making it a more complete and comprehensive whole, but it must not be lost sight of that the system has grown from a small beginning, with frequent and important additions, necessarily following one another in the best way possible.

At the new General Post Office the whole of the 750 wires are carried up the interior of the building and terminate on a test-box, where each wire is numbered. Provision has been made on this box for 500 wires from the west, and a corresponding number from the east, a total of 1,000 in all.

New Work.

A 4-inch pipe will hold 120 wires of No. 7 gauge prepared, and a 3-inch pipe 72 wires, but it is not well, unless compelled by circumstances, to draw in these numbers.

The pipes are 9 feet long, and previous to being laid are well cleaned inside by having a heavy chain or mandril drawn to and fro to rub off any superfluous substance left in casting.

The socket joints are packed with tarred yarn, and lead run in, as in the case of gas- or water-pipe joints. Great care is necessary to prevent the lead entering the pipe, and so damaging the wires when drawn in.

In marking out the route the footpath is generally chosen; the pipes are laid under the pavement, it being much more accessible than the roadway, especially since the asphalte has been introduced.

Flush-boxes 2 feet 6 inches long, 11 inches wide, and one foot deep, are fixed in some cases 50 yards apart, and in others 100 yards, according to the number of wires required and the nature of the streets.

A No. 8 galvanised iron wire is threaded through the pipes from box to box as the pipes are laid, by which the cables are hauled through.

All the cables are sent out from the Postal Stores in lengths of 400 yards. On arrival at the place where it is to be used each cable is coiled in a loop, a short distance from the centre flush-box of the 400 yards length; the ends of the wires are trimmed for about six inches; the cable is then divided into two, and each portion being twisted, they are then passed through the loop in the iron leading-in wire in contrary directions, bent back, and secured; a piece of canvas is then wrapped round and fastened with string.

An iron frame with two wooden rollers fitted is then fixed in the flush-box. The cable is made to pass over one roller and under the other. The latter is so arranged that the cable enters the pipe with a clear lead, and without being chafed against its edges.

All being now ready for the drawing-in, two men stand within the coil and cut the ties, delivering the cable to a third who is over the flush-box, and whose duty it is to see that the cable enters the pipe evenly. The foreman should be standing near so as to watch the cable entering the pipe, and to signal to the men at the next box when to commence or cease pulling.

When the first fifty yards of the cable have entered the pipe, (and this is known by the end appearing at the next box) a piece of tape is tied round the cable; when this has passed through the pipe a second piece is tied on, and so on till the two hundred yards have been drawn through.

As the cable comes out of the pipe it is coiled on the opposite end to that from which it has been drawn, the cable is then turned over by being coiled on the contrary end of the box, ready again to enter the pipe. This operation is repeated till the whole two hundred yards have been laid.

A corresponding operation has to be performed with the remaining two hundred yards of the cable, but, of course, in the opposite direction.

As soon as a few sections of the cable have been laid, the jointers follow, starting from the central station, St. Martin's-le-Grand, jointing the wires in the vaults under the pavement at the corner of Bath Street, Newgate Street, to the house-wires, leading from the test-box in the instrument room, and which pass through a rack numbered so as to correspond with the terminals on the test-box. Having completed the joints at that spot, one man proceeds to the next joint-box, 400 yards distant, and the other to the test-box; they then commence numbering the wires by putting a current on the lowest number through a galvanometer, and, when this is found by the man at the flush-box, three signals are passed twice each way; the wire is then fitted with a small piece of composite tube, on which a number has been stamped corresponding with the test-box number; every wire is numbered in this way at the 400 yards boxes, so that at any joint-box the number of any wire is at once known.

The men engaged at this work in London have had great experience, and, although the whole of their joints will not perhaps, to use their own phrase, "Stand the shadow test" (Thompson's reflecting galvanometer), they are generally good. The greatest enemy they have to contend with is dirt, and, although full instructions are issued and every care is taken, it is difficult to carry them out thoroughly in the streets of London.

The method of jointing gutta-percha wire was so fully described by Mr. G. E. Preece, in his paper on "Underground Telegraphs," read in November, 1873, that I will not occupy your time by repeating it. In the same paper a description was given of the manner in which the joints were tested, and the good results gained thereby. I believe the wires referred to by him still test well, and show that where it can be done it is proper that great care should be taken in testing each joint; but I fear the same system cannot be applied to the street-work of London, where we have something over 17,000 joints.

There have been several methods proposed for improving the joints in the streets, but as yet they have met with little favour from those engaged at the work; probably prejudice has had something to do in the matter. It has been the custom to make a twist-joint,

and now it has been suggested to insert the ends, after being cleaned, into a piece of slit copper tube, tinned on the inside, of about three-quarters of an inch in length, after which it is soldered. This makes a good joint, and I believe will prove beneficial, as it does away with the sharp points that must be left when a twist is made.

A plan for insulating joints is on trial at several places. The two wires are passed through the wooden bottom of a short tin tube, the twist is made and soldered, and then the wires are pulled back into the tube, and the latter is filled with melted paraffin wax. This plan was explained at a recent Meeting, and on a future occasion it is to be hoped that the results of these experiments will be made known. Such a system of mechanical jointing, of course, could not be used where it would be necessary to draw the joints through a pipe.

Maintenance.

Within the past five years nearly the whole of the underground system in London has been relaid, the number of wires having been found insufficient to meet the increased metropolitan traffic, as well as the additional wires rendered necessary on the railways and road lines for the rapidly developing provincial business.

This work has been effected with comparatively little or no interruption of the working circuits, certainly with far less than is experienced from renewals on railways or road lines.

The manner in which this work has been performed may not be uninteresting.

Take, for example, increasing the number of wires between St. Paul's Alley and Wellington Street, Strand, from 50 to 110 in the 4-inch pipe (provision having previously been made between St. Martin's and St. Paul's Alley for the increased number). The cable of 110 wires, 400 yards long, was coiled on the pavement at the flush-box half-way between St. Paul's Alley and Ludgate Hill Bridge, the two ends of the cable being brought into the box.

From a battery of six cells a current was passed through a galvanometer into one of the new wires, and the other end of the wire found by completing the circuit with the other pole of the

battery. Having found the wire and prepared the ends for jointing, one of the old working wires was trimmed and the copper cleaned for about three inches; one man taking one end of the new wire, and his assistant the other, both ends were put in connection with the working wire before it was cut, after which two bell-hanger's joints were made, thus putting the working wire through its new wire in the cable; this operation was repeated with all the working wires, and each joint temporarily covered with thin sheet percha.

The remaining sixty wires were then trimmed, and made fast round the working wires in advance of the joints, and a piece of canvas wrapped round to prevent abrasion.

To draw out the old cable, and the new one in, a rope was made fast round the cable at the next flush-box, and, as the cable was hauled through, it was coiled, as in drawing in a new cable; in fact, the same work had to be done as in new work.

When the whole of the new cable had been drawn in, there was a loop of two hundred yards at St. Paul's Alley, and also at Ludgate Hill Bridge, to cut out which the small battery was again used.

A wire was trimmed just below the temporary joint, and one pole of the battery connected to it through a galvanometer. A notch was cut in the percha of each wire at the opposite end of the loop so as to make the copper visible for about one-eighth of an inch, preventing the wires making contact, at the same time enabling the man to touch each wire and complete the circuit through the loop.

The battery being small and the deflection noted, there was very little danger of cutting the wrong wire. The wire having been trimmed and another temporary joint made, a current was again passed through the loop previous to its being put aside; in this manner the whole of the wires were cut out.

In some instances the new cable is looped in at a 400 yards box; this obviates cutting the old cable at every 200 yards, as was done in the work I have mentioned; but there is this disadvantage, the first portion of the new cable must be drawn through the entire 400 yards of pipe, an operation liable to injure it.

The underground wires are tested periodically from the central station by means of a Wheatstone bridge and a Thompson galvanometer; to prevent stopping the circuit two spare wires (where available) are used between St. Martin's and the point to which the tests are to be taken, one joined to an instrument for a speaking circuit, and the second for substituting the working wire during the time it is being tested. The average insulation is about twelve megohms per mile.

From these tests it is readily seen when a wire is getting low in insulation, and it frequently happens that the cause is detected and remedied before it is sufficiently bad to interfere with the working.

By means of a loop test, the position of a fault is traced, and it is seldom that the tests are wrong, generally within a 400 yards length.

Having the resistance between St. Martin's and the several junction boxes recorded, the position is known as soon as the test is taken, and men are sent off to find the cause.

In the event of two faults coming on a wire at the same time, the resistance of the loop would show that such was the case, as it would offer *less* resistance than that recorded.

The number of faults reported as interruptions of circuit are very few indeed.

Occasionally the joints have been found defective, but more frequently the defect has proved to be within the pipe, and such faults must arise from one of the three following causes: Friction against the pipe when pulled in; a bad joint in cable made before sent out from the stores; lightning.

The last-named I believe to be one of the chief sources of faults. A short time ago, previous to lightning protectors being fixed on two duplex circuits at the Stock Exchange, faults were reported, and when discovered there was every reason to believe they had been brought about by lightning. In one case the wire was badly centred, probably owing to the jointer allowing it to pass out of his hands before it was cool. The second case appears to have been at the end of the twist, one end of the copper wire probably being left sticking up, having a sharp point. If this view be correct, it would be well to have some kind of test-box at the end of each underground route, where lightning protectors could be fitted.

A few years back one of the chief causes of interruptions was from a bad practice of cutting or pricking wires. When a wire was wanted at a box where numbers had not been attached, a current was sent through it, and each wire cut with a knife, having a wire fastened to it joined to earth through a galvanometer. As soon as the blade touched the conductor of the right wire the needle was deflected. Another mode of doing this was by using a small bradawl, to which the wire from the galvanometer was joined; the percha was pierced till the copper was touched. Experience showed that both these practices were highly dangerous, many cases having been discovered where the incisions had not been made permanently good. This was improved upon by having a small piece of percha cut out, so as to insure the wire being made good. I am glad to say we now find it wholly unnecessary to continue these bad habits.

During the latter portion of the time we were in the old building at Telegraph Street it frequently happened that a wire was wanted out of a mass where there was nothing to identify it by; rather than prick them, I endeavoured to find the wire by using a wire-finder (such as mentioned by Mr. Culley in his "Handbook of Practical Telegraphy"), but found it a very difficult task, owing to the currents in the working wires affecting the needle. Eventually it struck me, that, if I used a quantity-current and the horizontal galvanometer generally used for the Wheatstone Bridge, I should succeed. It proved to be correct—the quantity-current only moving the needle. Since that time, when occasion has required, it has been used in the streets, and answers admirably. The reason is obvious. The highest resistance of any of our street wires is about 300 ohms, whereas that of any working circuit is much greater. The result is, there is not sufficient quantity passing through the latter to move the needle.

In conclusion, I venture to hope that we may see the underground system extended far beyond the outskirts of London, believing as I do that, if the same care and attention were given to it as is the case with our submarine cables, it would prove to be a great success.

Mr. C. F. Varley: To a great many Members of this Society the process of laying and renewing underground work is perfectly familiar, but we have continually an accession of new Members to whom this work is entirely strange, and I for one regret on their behalf that more diagrams were not produced to illustrate the methods of renewal. Very little mention was made of the earliest system of underground work used by the Electric Telegraph Company, and we ought not to lose sight of it, because, should the supply of gutta-percha by any accident fail us, I do not know any other system more promising of success. With the knowledge and the appliances we have at the present day it would be quite possible to lay down wires, not perhaps very highly insulated, but sufficiently so for all the practical purposes of telegraphy.

I will therefore give a brief description of the method adopted in those days, in order that you may fully understand it. The wires were covered with two thick layers of cotton in opposite directions. Nine of these wires (there should have been seven or thirteen to make a circle, but they used nine) were drawn into a leaden tube, and the error was made of cutting slits in the leaden tube at every six yards, in order that when the tube and the wires were placed in a cauldron, containing a mixture consisting chiefly of wood, pitch, rosin, and bees'-wax, this mixture should run into the tube.

Nearly the whole of that work was laid by myself, and I protested greatly against the slits, because in soldering them up again either the lead or the solder ran in between the wires and frequently damaged them, or else the soldered joints cracked during the coiling and uncoiling of the pipes, the solder being much more brittle and much harder than ordinary lead. I had a few pipes (I had great difficulty to get permission to do it)—I had a few pipes made without the slits, and after they had been boiled for twenty-four hours, the usual time, the pitch, rosin, and wax compound was found to have penetrated throughout the entire length of fifty yards. Had the pitch mixture been prepared beforehand by boiling a sufficient length of time to expel all the acid and all the water, these tubes would have been very durable, but, as it was, the minute quantity of water, or acid and water, which remained, seemed gradually to collect together under the influence of the

electric currents, and ultimately to connect the wires so effectually that they became too leaky for the purpose required. I mention this particularly, because for about four years the whole of the communication between the North of England and London was conducted through some sections by means of wires insulated in this manner—notably through the tunnels on the London and North Western Railway and also between the Shoreditch Station and Lothbury. The first communication with the North was by means of wires on the Eastern Counties Railway. It is therefore evident that this is a practical method of telegraphy, and I have seen good intelligible signals come right through from Edinburgh to London through wires insulated by these means in many of the tunnels and in the streets of Edinburgh and London, and before gutta-percha was used for street or underground wires.

The next subject which has struck me in connection with this paper, which unfortunately I have not had time to read beforehand, was the proposal of insulating the wires, after being united, by means of paraffin in a copper tube. It is well worthy of note by all Members that paraffin and copper do not often agree, any more than do copper and india-rubber, and therefore, unless the copper wire be previously tinned throughout that length which is to be exposed to the paraffin—and that cannot be the case unless the whole wire from end to end is tinned—there will be an action between the two which is likely to cause their destruction. Nevertheless this method of producing a rapid means of insulation deserves following out, and I have but little doubt that the skill of those engaged upon this work will ultimately overcome the difficulty in question. For example, if rosin be mixed with paraffin, the action on copper is very much reduced, and it is quite likely that by the admixture of other gums a compound of sufficient insulating power may be found which has no reaction with the copper. It is well known that copper wire inside ordinary india-rubber is almost certain in the course of a few years' time to decompose the caoutchouc into a compound of the consistence of treacle, and this action or reaction between the two has destroyed many and many a mile of wire insulated by india-rubber; it should be observed that the rubber in all these cases was joined by means of coal-tar naphtha.

I understood it to be stated in the paper that bellhanger-joints were used when they made a loop, as for example when they were going to cut a wire to draw out one section and put in another. As I explained on a previous occasion, the two ends of the loop were rolled round the wire to be cut, and then the main wire was cut and the electric circuit went round through the loop. By repeating this operation on each of the wires the loops were gradually inserted until all the circuits were so diverted. But in the paper the words "bell-hanger joints" were made use of. I need not remind you that bell-hanger joints, that is to say, one loop within another, are never used, because the slightest shifting of one from the other would break the electric circuit.

The fourth note which I made during the reading of the paper refers to faults arising from lightning. It should be distinctly understood that the underground wires were not themselves struck by lightning, but the land lines—the overground lines upon the railways in connection with these underground lines—were struck, and the lightning ran into the underground wires to find a vent to the earth.

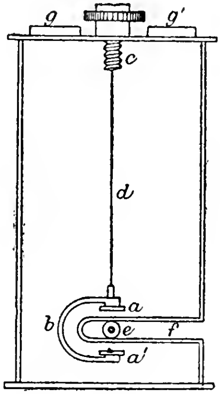
Next, as to gas in the pipes. It has been stated that smokers frequently, by striking their cigar-lights against a post in the street, caused explosions of the gas which escaped from the defective gas mains into the telegraph pipes. I think those words are a little too strong. It should have been "sometimes caused explosion;" and for this simple reason—that a mixture of coal gas and atmospheric air, when there is too much gas, will not explode—for example the Bunsen burner—and it requires about 15 volumes of air to one of gas in order to make an explosive mixture. Now, the gas companies' pipes are so extremely leaky, that the quantity of gas escaping from the gas pipes into the telegraph pipes, and thence from the pipes to the posts, was generally so great that the gas would burn at the keyhole. This was notably the case at the post at the corner of the Euston Road and Seymour Street, which was well known to the cabmen and others, and they used to take great pleasure in lighting the gas at the keyhole of the post. In addition to this, I do not think the action of gas upon the wires at all conduces to the soundness of their constitution, and if we could by any possible

means prevent the introduction of gas, and consequent deposition of hydrocarbon from the gas, no doubt the London street wires would last a longer time without repair or renewal than they do.

The next point to which I would refer is that of pricking wires, to find which is the wire sought for. Years ago it was the custom to apply a battery, one pole to earth and the other to the wire sought for—say at the central station; by this means the battery was endeavouring to pass a current through the wire to earth. If then a galvanometer, one of whose poles was connected to earth and the other to a bradawl, was put in connection with the wire, by inserting the bradawl through the gutta-percha a current of electricity flowed immediately the wire was discovered. This method, I take for granted, has long ceased to be adopted by the Post Office. It has been abolished and entirely forbidden under severe penalties in all cable factories. Now, there is no difficulty whatever in selecting a wire from the outside without injuring the gutta-percha. We are all familiar with the common electrical experiment of passing a current through a wire under or over a needle, which is deflected by it; and in 1855 I made an arrangement which will be found, I think, in one of my early patents, by which this could be done easily. It is as follows:—

Supposing a battery to have one pole in connection with the earth, and the other to be connected with the wire to be sought for, and let this wire also be connected to earth at its distant end, then the resistance of this short section—say one or two miles—being very slight, the current passing through the wire will be very great, as all acquainted with Ohm's laws well know. If an astatic pair of needles be now placed, the one above the wire and the other below it, the needles being suspended from a thread, and if these two are nearly balanced, they will be deflected when the right wire with its powerful current is placed in the apparatus. Accordingly, I had a box made (*vide* accompanying diagram) with an indentation on one side, passing rather more than half through it. From the centre of the lid of the box came a thread attached to a small piece of metal carrying the needles, which were connected with each other by a curved piece of brass. If now a wire be placed inside the slit, and if there be a powerful current passing through

it, the needle will be deflected, as everybody knows perfectly well; and by this means I found no difficulty whatever in selecting the



a a' magnets with poles in opposite directions.
b connecting piece.
c adjusting screw.
d silk suspension.
e section of wire under test.
f slit in the box to insert wire *e* to be tested.
g g windows to inspect needles and see if they are deflected.

wires as fast as they were required. That method is so simple and so easily understood by everyone that I recommend it for adoption.

I should mention, before I resume my seat, that a great many experiments were made in London with different insulating materials, notably with one called Wray's mixture. This consisted of india-rubber, gutta-percha, French chalk, and finely-powdered silica. This material at first insulated admirably, and its inductive capacity was very considerably less than gutta-percha, but at the end of six months' time we found the insulation was gradually decreasing, and it kept on decreasing for a considerable period of time. This decrease of insulation was found on careful analysis to be due to the absorption of water by the insulating material, and therefore it was unsuited for street work; but this material on the other hand was well adapted for indoor or office work. When gutta-percha covered wires are used in an office and exposed to a dry atmosphere and burnt gas, the gutta-percha gradually becomes brittle, and looks more and behaves more like tobacco-pipes than gutta-percha. It gradually contracts and separates in short

lengths, which are hard and brittle, leaving the wire exposed; the gutta-percha contracting 20 or 25 per cent. in a short space of time. India-rubber has, unfortunately, the property which I described of becoming treacly, and occasionally the india-rubber runs away entirely, and leaves nothing but the plain bare copper wire. Wray's material, on the other hand, only very slightly softens by this process. It does not become brittle, and in a dry atmosphere preserves its insulating power, and I think it will yet do good service in large stations, such as the General Post Office, because it will not be liable to the failure to which gutta-percha is. At first sight this drying of the gutta-percha may not appear a serious evil, since the wires in an office are dry, and so long as the channel in which the wires pass remains dry the brittle character of the gutta-percha and the fact that the gutta-percha has separated and left the wire exposed are of no consequence, seeing that there is no better insulator than the atmosphere; but, when from any accident water happens to be thrown down the channel containing the wires, every one of those dry and cracked wires is thrown into what is technically called "contact," and telegraphing becomes impossible. This happened more than once at Telegraph Street, and threw nearly every circuit into a state of confusion for a considerable period of time.

Mr. W. H. Preece: I should like to ask Mr. Varley if he can give us any intelligible reason why there should be reaction between paraffin and copper, and between india-rubber and copper?

Mr. Varley: It is well known that all fatty substances and copper react upon each other. For instance, if you place a plate of copper at the bottom of a vessel, and fill that vessel with oil or fat, you will see the copper gradually ascend by its green colour through the solid fat until it reaches the top of the vessel. In the same way, if you place some sulphate of copper at the bottom of a vessel containing common glue you will see the copper ascend through the common glue as rapidly as it would through plain water. The effect is to produce what is known as setacic acid, but whether setacic acid is produced in india-rubber and paraffin I am not prepared to say.

The further discussion was then adjourned till next Meeting.

The SECRETARY announced the result of the ballot for President and Members of Council for the ensuing year to be as follows:—

President.

C. V. WALKER, F.R.S.

Past-Presidents.

LATIMER CLARK, M.INST.C.E.
SIR WILLIAM THOMSON, F.R.S., LL.D.
FRANK IVES SCUDAMORE, C.B.
CHARLES WILLIAM SIEMENS, F.R.S., D.C.L., M.INST.C.E.

Vice-Presidents.

PROFESSOR ABEL, F.R.S.
MAJOR J. U. BATEMAN-CHAMPAIN, R.E.
R. S. CULLEY, M.INST.C.E.
PROFESSOR G. C. FOSTER, F.R.S.

Members.

PROFESSOR W. G. ADAMS, F.R.S.
H. G. ERICHSEN.
COLONEL GLOVER, R.E.
EDWARD GRAVES.
CHARLES HOCKIN, M.A., C.E.
MAJOR MALCOLM, R.E.
W. H. PREECE, M.Inst.C.E.
ROBERT SABINE, C.E.
CARL SIEMENS, M.Inst.C.E.
C. E. SPAGNOLETTI, M.Inst.C.E.
LIEUT.-COLONEL STOTHERD, R.E.
C. F. VARLEY, F.R.S., M.Inst.C.E.

Associates.

OLIVER HEAVISIDE.
W. J. TYLER.
JAMES SIVEWRIGHT, M.A.

OFFICERS.

Auditors.

J. WAGSTAFF BLUNDELL.
FREDERICK C. DANVERS (India Office).

Hon. Treasurer.

MAJOR C. E. WEBBER, R.E.,
Telegraph Street, E.C.

Hon. Secretary.

MAJOR FRANK BOLTON,
4, Broad Sanctuary, S.W.

Hon. Solicitors.

MESSRS. WILSON, BRISTOWS, & CARPMAEL.

Secretary.

GEO. E. PREECE.

The following Candidates were balloted for and declared duly elected :—

As Foreign Members :

Don Antonio Oloriz.
L. Arisz.
C. Brieve.
H. Nielson.

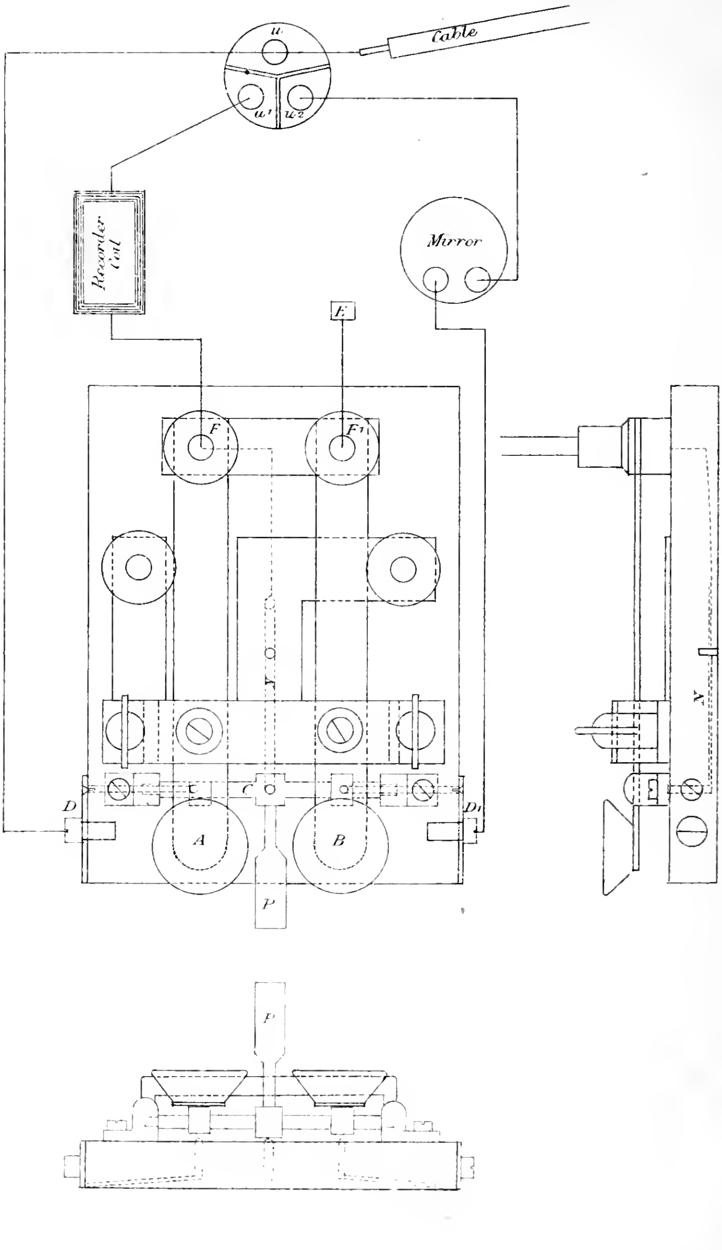
As Members :

J. Ahern.
W. C. Johnson.
J. F. H. Betts.

As Associates :

E. Castle.
G. Dubern.
G. W. Hook.
W. Judd.
J. J. Philpott.
J. J. Payn.
W. N. Tiddy.
R. A. Warner.
H. Yates.
F. Kenney.
A. Richards.
J. Oldershaw.
E. Emms.
G. Morrison.
J. Ross.
W. Turner.

The Meeting then adjourned.



Cable
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ORIGINAL COMMUNICATIONS.

NEW ARRANGEMENT OF KEY FOR CABLE WORKING.

To the Secretary of
The Society of Telegraph Engineers.

SIR,—Accompanying this is a hasty drawing of an improvement I have made. It acts perfectly. You will see at a glance that the switch is thrown entirely into the key. The advantages are, that the clerk cannot send until the handle is in proper position, and should recorder break down the messages coming can be taken on mirror, with only the loss of one or two words. It simply requires the removal of a plug from one hole to another.

If you think this worthy of mention before the Society, I shall be glad if you will do so.

I remain, Sir,
Yours obediently,
F. DEWAR.

Rio Janeiro.

Instructions.

The cable is connected to one terminal u of a three-wayed commutator. The terminal u_1 is connected line of recorder. The other end of coil is connected to terminal F of key. When the lever P is perpendicular it is for receiving, the two levers A and B are slightly pressed against the bridge of key, preventing the clerks depressing them. In sending, the lever P is horizontal. The cam S is then in contact with spring leading to terminal D, which is in connection with terminal u of cable. From terminal F of key A a permanent connection is made to spring N, which acts as a check to cam S, holding it in its right position for sending and receiving. In sending, when (on recorder) the lever B is depressed, the current running along lever A divides at F, the greater portion going through spring N to cam-shaft C to spring D (which is in con-

nection with cam-shaft C), meeting coil portion at terminal u. When receiving, the handle is perpendicular, connection is broken between D and cam-shaft C, letting the whole current pass through the coil to earth.

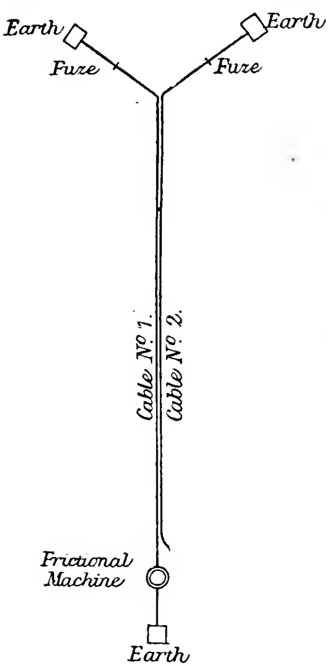
When on Mirror.

Simply requires the plug to be removed from between u and u_1 to u and u_2. The mirror can then be worked in a similar manner to recorder.

EXPERIMENTS TO ASCERTAIN THE EFFECT OF FIRING A MINE BY ELECTRICITY ON CONDUCTING CABLES AND FUZES IN ITS VICINITY.

By Lieut.-Colonel STOTHERD, R.E.

The following experiments, tried at Chatham in 1870, may be interesting, as considered in connection with Mr. Culley's paper on "Induction between Suspended Wires."



Two electric cables, each consisting of a strand of seven No. 22 copper wires, insulated with Hooper's dielectric to a diameter of $3\frac{1}{10}$-inches, were laid side by side on dry ground for a distance of half a mile, but separated at their outer extremities by a distance of about twenty yards (*vide* accompanying diagram). An ordinary Abel's mining fuze was attached to the outer extremity of each cable, and a separate earth connection established beyond each fuze by means of a short connecting wire and earth-plate immersed in the soldiers' bathing pond. The water of this pond is brackish, being supplied from a tidal river, the Medway, at Chatham.

A series of fuzes was fired through one of these conductors, to be called, for distinction, No. 1, by means of a field-service Austrian ebonite frictional machine. While the fuzes on No. 1 conductor were thus fired directly, those on No. 2 conductor were fired under the following conditions:—

1st. With both ends of No. 2 cable to earth, viz., that at the firing station and that beyond the fuze.

2nd. With the end of No. 2 cable at the firing station insulated, and the connection beyond the fuze to earth.

3rd. With the end of No. 2 cable at the firing station to earth, and a second Abel's fuze, representing a considerable electrical resistance,* introduced between the conductor and earth connection at this point, the connection beyond the fuze at the outer extremity of the cable being, as before, to earth. In this case both fuzes, one at each end of No. 2 cable, were fired.

The same results were obtained, under the three conditions above specified, when two fuzes in continuous circuit were substituted for one at the outer extremity of No. 2 cable, and again, where three fuzes were placed in continuous circuit at the same point. These last experiments were tried to ascertain whether the introduction of a considerable electrical resistance into the circuit would prevent the indirect firing of the fuzes. With two fuzes the total resistance would be as nearly as possible doubled, and with three fuzes trebled, the comparative resistance† of the conductor being insignificant.

The cables were subsequently arranged at various distances, from 3 feet to 40 feet apart, the conditions above enumerated being retained, and the experiments repeated. The results of these experiments will be found in the table attached.

Again a fuze was introduced in No. 2 cable, with three yards of insulated conductor beyond it, the end of this short length being insulated, and the cables lying parallel to each other for half a mile, and three feet apart. Under these circumstances the fuze on No. 2 cable was fired with the same certainty as before.

The object of this last experiment was to ascertain the effect on a

* The electrical resistance of an Abel's mining fuze (composed of subsulphide of copper, subphosphide of copper, and chlorate of potash) is from 1,500 to 2,000 ohms.

† The resistance of such a conductor would be about 12 ohms per nautical mile.

fuze with a circuit-closer beyond it. The connection with the circuit-closer was represented by the three yards of cable insulated at its outer extremity.

Subsequent experiments were made with a battery of 100 Daniells' cells, with Siemens' Dynamo-Electrical machine,* and with Wheatstone's Magnetic Exploder, but the fuze on No. 2 line was never fired, even under the most favourable circumstances, in using these three latter firing agents.

The electric cables (Nos. 1 and 2) employed were tested for insulation, conductivity, and resistance, before the experiments were made. The means of testing available at that time at the School of Military Engineering were somewhat rough as compared with the delicate apparatus now employed; they were, however, sufficient to show that the cables, which formed a part of the field electric telegraph equipment, were in good working order; that is to say, that the insulation, conductivity, &c. would have been sufficient for ordinary telegraphic purposes.

The results obtained led to the supposition that the fuzes on No. 2 cable were fired by induction. Subsequent experiments, made by Major Malcolm and Lieutenant R. G. Scott, R.E., throw an entirely different light on the subject. A record of the results of these latter is appended.

Experiments made in Austria have proved that when a return wire is used frictional electricity may be employed to fire high tension fuzes without danger of ignition to fuzes in connection with cables in the vicinity of that through which the direct current may be discharged. With this modification the Austrians continue to use frictional electricity for the ignition of submarine mines in certain cases (see pp. 413-15).

Experiments carried out under the following conditions:—

Two air lines, each 825 yards long, of No. 16 galvanised iron wire, well insulated, were separated by an average distance of $6\frac{1}{2}$ inches. A No. 1 Electric Abel (old service pattern) was inserted into the upper line, of which both ends were to earth, with a Von Ebner Frictional Machine in circuit, while the other line had the

* The Dynamo-Electrical machine used was one of the first supplied by Messrs. Siemens to the War Department, and weighed about 28 lbs.

TABULAR STATEMENT OF RESULTS OF EXPERIMENTS.

No. of Experiment.	Distance of Cables apart.	Firing Apparatus employed.	Number of turns given in charging Frictional Machine.	Number of Fuzes in each Cable and Connections.	Results.
1	Touching	Frictional machine, field service pattern (ebonite discs)	50	One on each cable at outer extremity. Both ends of No. 2 cable to earth	Both fuzes fired, one directly on No. 1 cable, the other indirectly on No. 2. This result was repeated several times under the conditions specified
2	Do.	Do.	50	One on each cable at outer extremity. The end of No. 2 cable at firing station being insulated and outer end to earth	Both fuzes fired, one directly on No. 1 cable, the other indirectly on No. 2. This result was repeated several times under the conditions specified
3	Do.	Do.	50	One fuze on No. 1 cable at its outer extremity. One fuze on No. 2 cable at its outer and one at its inner extremity, fuze and both ends to earth	All three fuzes fired, one directly on No. 1 cable, the others indirectly on No. 2. This result was repeated several times under the conditions specified
4	Do.	Do.	50	Arrangements similar to Experiment 1, two fuzes and subsequently three fuzes being used on No. 2 cable	Fuzes on No. 2 cable all fired in every case
5	Do.	Do.	50	Arrangements similar to Experiment 2, two fuzes and subsequently three fuzes being used on No. 2 cable	Fuzes on No. 2 cable all fired in every case
6	Do.	Do.	50	Arrangements similar to Experiment 2, two fuzes and subsequently three fuzes being used on No. 2 cable	Fuzes on No. 2 cable all fired in every case

No. of Experiment.	Distance of Cables apart.	Firing Apparatus employed.	Number of turns given in charging Frictional Machine.	Number of Fuzes in each Cable and Connections.	Results.
7	3 feet	Frictional machine, field service pattern (ebonite discs)	50	Arrangements similar to Experiment 1	Fuze on No. 2 cable fired in every case
8	3 feet	Do.	50	Arrangements similar to Experiment 2	Fuze on No. 2 cable fired in every case
9	3 feet	Do.	50	Arrangements similar to Experiment 3	Fuzes on both cables fired in every case
10	Touching	Do.	50	One on each cable at outer extremity, with 3 yards of electric cable beyond fuze on No. 2, outer end insulated	Fuzes on both cables fired in every case
11	3 feet	Do.	50	Arrangements similar to Experiment 10	Fuzes on both cables fired in every case
12	3 feet	Do.	4	Arrangements similar to Experiment 2	Fuses on both cables fired
13	3 feet	Do.	4	Ditto	Fuze on No. 1 cable fired. Fuze on No. 2 not fired
14	6 feet	Do.	20	Ditto	Fuzes on both cables fired
15	9 feet	Do.	20	Ditto	Fuzes on both cables fired
16	12 feet	Do.	20	Ditto	Fuzes on both cables fired
17	15 feet	Do.	10	Ditto	Fuze on No. 1 cable fired by direct action. Fuze on No. 2 cable not fired
18	15 feet	Do.	20	Ditto	Fuze on No. 1 cable fired by direct action. Fuze on No. 2 cable not fired
19	15 feet	Do.	30	Ditto	Fuzes on both cables fired
20	20 feet	Do.	20	Ditto	Fuze on No. 1 cable fired by direct action. Fuze on No. 2 cable not fired
21	20 feet	Do.	30	Ditto	Fuzes on both cables fired
22	30 feet	Do.	30	Ditto	Fuzes on both cables fired

No. of Experiment.	Distance of Cables apart.	Firing Apparatus employed.	Number of turns given in charging Frictional Machine.	Number of Fuzes in each Cable and Connections.	Results.
23	40 feet	Frictional machine, field service pattern (ebonite discs)	30	Arrangements similar to Experiment 2	Fuze on No. 1 cable fired by direct action. Fuze on No. 2 cable not fired
24	40 feet	Do.	40	Ditto	Fuze on No. 1 cable fired by direct action. Fuze on No. 2 cable not fired
25	40 feet	Do.	50	Ditto	Fuze on No. 1 cable fired by direct action. Fuze on No. 2 cable not fired
26	40 feet	Do.	50	Ditto	Fuze on No. 1 cable fired by direct action. Fuze on No. 2 cable not fired. The fuze on No. 2 cable was changed on this occasion to ascertain whether the failure to fire arose from a defective or less sensitive fuze
27	35 feet	Do.	50	Ditto	Fuze on No. 1 cable fired by direct action. Fuze on No. 2 cable not fired
28	30 feet	Do.	50	Ditto	Fuzes on both cables fired
29	Touching	Dynamo-electric machine, Siemens, 28 lbs. in weight	...	Ditto	Fuze on No. 1 cable fired by direct action. Fuze on No. 2 cable not fired
30	Do.	Do.	...	Ditto	Fuze on No. 1 cable fired by direct action. Fuze on No. 2 cable not fired
31	Do.	Wheatstone's Magnetic Exploder	...	Ditto	Fuze on No. 1 cable fired by direct action. Fuze on No. 2 cable not fired
32	Do.	Do.	...	Ditto	Fuze on No. 1 cable fired by direct action. Fuze on No. 2 cable not fired
33	Do.	Battery of 100 Daniell's cells	...	Ditto	Fuze on No. 1 cable fired by direct action. Fuze on No. 2 cable not fired
34	Do.	Do.	...	Ditto	Fuze on No. 1 cable fired by direct action. Fuze on No. 2 cable not fired

near end insulated, the far end to earth, and a No. 1 Electric Abel fuze near far end, close to fuze in upper line. The distant earth separated about twenty-five yards; the distant earth for top line being soldered to a gas-pipe near the main, and the bottom, a copper plate in very moist earth.

To determine whether it was possible to explode the fuze in the bottom line by induction, from a charge passing through upper line, and exploding its fuze.

This was tried as above ten times, and in no instance was the fuze in lower line exploded.

The lower line was then put to the same earth as upper line at home (or near end), the earth being a copper earth-plate in moist earth, the outer earth and fuzes being the same as before.

With Von Ebner both fuzes exploded.

Under the first conditions, the Holtz Induction Machine in good working order, giving a spark of four inches, was applied to line—

1st. Without fuze in upper line;

2nd. With fuze on each line;

3rd. With the lower end at home to the same earth as with frictional machine.

Fuzes in upper line exploded, but none in the lower line, even when the lower line was put to earth at the home end.

With the Holtz machine, two small Leyden jars sent with the machine were used; the jars are $5\frac{1}{2}'' \times 1\frac{3}{8}''$, filled with gold leaf, with tinfoil covering two inches from the bottom outside. Showing, therefore, that the inductive effect was not sufficiently great to explode the second fuze, either from a Von Ebner or Holtz machine.

The examination was carried further, as shown more at length in the tabular statement which is attached (see p. 417).

E. W. Malcolm,
Major R.E.

Name of exploding instrument and if air or insulated lines.	1	2	3	4	5	6	7	8	9	10
	With Electric Abel No. 1 in circuit.									
	With two Leyden jars (of greater capacity than those sent with the machine) 8″ × $3\frac{1}{2}$″, with tin-foil covering inside and outside $5\frac{1}{2}$″ from bottom. Top line to earth at both ends; bottom at far end; home end insulated.	Home end of line disconnected from earth, with four Leyden jars of same capacity as in column No. 1, with a wire instead of a fuze in *top* line at far end.	With a fuze on *top* and *bottom* lines, both being to earth at far end. The bottom line at home end (not being to earth) insulated.	Another fuse put in bottom line and to earth, leaving the exploded fuze in *top* line at far end. Top line earths as before.	A fuze put in on each line at far end, and earths cut off at that end.	Bottom line at far end put to earth, leaving fuze in top line insulated, with a fuze in at that end.	*An electric No.* 3 *platinum wire fuze* put in bottom line, still to earth at far end; top line with fuze being insulated at far end; bottom line at home end being insulated, and top line on Holtz machine with four Leyden jars or other firing machine, other terminal of machine being to earth.	*An iridio platinum wire fuze* put in circuit in bottom line, instead of the electric No. 3 platinum wire fuze; other conditions being the same as in column No. 7.	Top line put to earth, and a galvanometer in bottom line also to earth, the bottom line at home end being insulated, the top line being to earth through Holtz machine with four Leyden jars; other firing machine as below.	Galvanometer removed, and an *Electric Abel No.* 1 put in bottom line. The earth at far end of top line was brought close to top line, leaving a gap in the upper circuit between the top line and earth of half an inch, the electric Abel No. 1 in bottom line being to earth.
Holtz Induction Machine and air lines	Two distinct explosions; first top, second bottom	Bottom fuze did not explode	Both exploded; first top, second bottom	Bottom fuze exploded	No explosion	Bottom fuze exploded	No explosion	Iridio fuze fuzed	No deflection on galvanometer	The fuze exploded, sparks heard at gap in top line
Von Ebner Frictional Machine and air lines	Top fuse only exploded	Do.	Top fuze only exploded	Do.	Do.	Do.	Do.	No result	Do.	No explosion and no sparks
Von Ebner Frictional Machine and two half-miles of insulated cable. Insulation value many millions ohms.	Do.	Do.	Do.	Do.	Do.	Did not explode	Do.	Top fuze exploded N.B.—This is a very doubtful result, and the experiment will shortly be repeated	Do.	Do.

EARTHS:
- Home end .. .. Copper plate in wet ground.
- Far end .. .. Top line soldered to gas pipe *near main*; Bottom line copper plate in moist earth } Distance between top and bottom earths 25 yards.
- Equipment cable .. Mud and water in ditches.

D'ARLINCOURT'S RELAY.

By R. S. Brough.

Although M. d'Arlincourt's Relay was exhibited in the Vienna Exposition of 1873, and had already been in use for some years in France before that, no description of it, so far as I am aware, has as yet been published in England. It is, however, unquestionably novel in principle and successful in practice.

The novelty of the Relay in principle is attested by the fact that in a description of it published in 1872 in the "Journal Télégraphique" of Berne its action was misinterpreted, while in his report to the French Administration on the Vienna Exhibition M. Clérac says, "Cette seconde armature oseille, comme la première, sous l'action directe du courant de ligne, mais, lorsque ce courant cesse, elle effectue encore un dernier battement sous l'influence d'une réaction magnétique, *dont la cause n'a pas, jusqu'ici, été complètement expliquée.*" Subsequently, however, Mr. Schwendler in India and M. Du Moncel in France have correctly explained its action.

(1.) *Preliminary.* If the soft iron rod A B (fig. 1) be wound with insulated copper wire in the way shown in the figure, and a current be passed through the wire in the direction indicated by the arrow, then the core will assume a south polarity at A and a north polarity at B. If, further, the wire be wound continuously

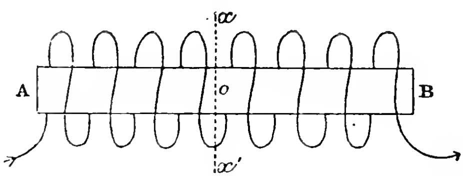


Fig. 1.

and systematically along A B, and if the rod be uniform and homogeneous throughout its length, then there will be a *single* neutral line $x\,o\,x'$ passing through the centre o of the electro-magnet at

right angles to its axis, and the whole of one-half A o of the rod will be south, while the whole of the other half B o will be north.

If, however, the wire be wound symmetrically but not continuously (the middle part of the bar being left uncovered) as in fig. 2, then there will be *three* neutral lines, viz. $x\,o\,x'$ (passing

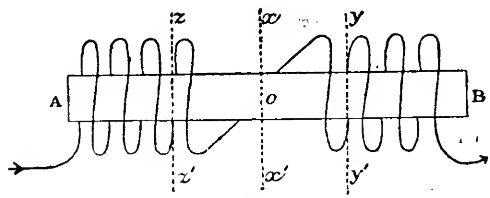


Fig. 2.

as before through the middle point of the rod); $y\,y'$ (passing through the right-hand helix, and lying nearer to o than to B); and $z\,z'$ (passing through the left-hand helix, and lying nearer to o than to A). From A to $z\,z'$ the rod will be south; from $z\,z'$ to $x\,o\,x'$ north (weaker than the former); from $x\,o\,x'$ to $y\,y'$ south (equal to the latter); and from $y\,y'$ to B north (equal to the first).

As the bare space intervening between the two helices diminishes, so do the two neutral lines $y\,y'$ and $z\,z'$ (fig. 2) approach towards the principal neutral line $x\,o\,x'$; and they finally coincide with it when the rod is covered with wire throughout (fig. 1).

Consider the case represented in fig. 2. So long as the current continues to flow the polarity will be distributed in the manner indicated above. Suppose, however, we interrupt the current, what will take place at the moment of interrupting it? *The action that supervenes is that which has been taken advantage of by M. d'Arlincourt, and which constitutes the novelty of his Relay.* It is as follows:—

The outer poles are stronger (as already stated) than the inner poles, and hence, at the moment of interrupting the current, the residual magnetism due to the outer poles will be greater than that due to the inner poles and will overcome it. Further, the outer poles have a greater distance to travel to neutralise one another than the inner poles, and will consequently outlive them. Hence, when the current is interrupted, the neutral lines $y\,y'$ and $z\,z'$ will

rapidly approach towards the principal neutral line $x\,o\,x'$, and coincide with it, thus leaving the half A o of the rod temporarily magnetised south throughout its length, and the half B o temporarily magnetised north throughout its length.

We see, therefore, that *directly the current is interrupted the polarity of the core lying immediately on either side of the principal neutral line $x\,o\,x'$ is temporarily reversed.*

Bearing the foregoing principles in mind, we will now examine the construction and working of the Relay.

(2.) *Construction of the Relay.* (See fig. 3.)

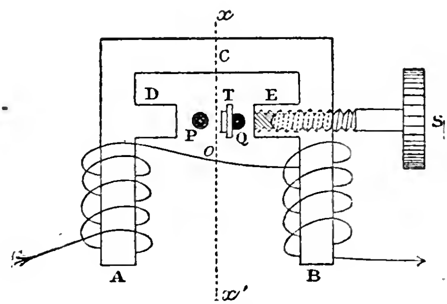


Fig. 3.

A C B is of soft iron, forming the core of a horseshoe electromagnet, wound in the manner indicated. D and E are projections to bring the core near the tongue.

T is a horizontal section of a vertically pivoted soft iron tongue, polarised by a large permanent magnet, and free to play between the stops P and Q.

P and Q are respectively the "local contact" and the "rest" stops.

S is an adjusting screw of soft iron for regulating the sensibility of the instrument, by varying the mass of iron present in the projection E.

$x\,o\,x'$ is the principal neutral line of the electro-magnet.

We shall suppose the end T of the tongue to be a *south* pole.

(3.) *The Relay used as a "receiving" instrument.*

Suppose the stops P and Q to be adjusted so that they both lie to the *right* of the neutral line $x\,o\,x'$. (See fig. 3.)

The tongue T is attracted by both the projections D and E, but since it is necessarily nearer to E than to D it will rest in contact with the rest-stop Q.

Now, suppose a current to flow through the wire in the direction indicated by the arrows. Then A will become a south pole, and B a north pole; D a weaker north pole, and E a weaker south pole.

Hence, since T is a south pole, T will be attracted by D and repelled by E; and, if the resultant force be sufficiently great, T will move over and rest in contact with the local contact stop P, and remain there so long as the current continues to flow.

When the current is interrupted, T tends by its own inductive action (since it is nearer E than D) to return to Q; but, over and above this, the moment the current is interrupted D is changed to a south pole and E to a north one, so that T is now attracted towards E and repelled from D. Under the influence of these forces, if sufficiently strong, the tongue T will return and rest against Q, when it will be ready to be moved by a fresh current.

Here I must remark, that, instead of the residual magnetism being a retarding influence, M. d'Arlincourt employs it as his "antagonistic" force. Hence the rapidity of action of this Relay. Further, the stronger the line current received, the stronger also the residual magnetism, and consequently the stronger the antagonistic force. Hence this Relay works almost without re-adjustment.

These are the two qualities that commend this Relay, and the value of which has been practically proved in France and in India.

(4.) *The Relay used as a line "discharging" instrument or as a "zinc-sender."*

If, instead of adjusting the stops P and Q so that they both lie to the *right* of the neutral line $x\,o\,x'$, we adjust them so that they both lie to the *left* of the neutral line, then normally, *i.e.* when no current is passing, the tongue T will rest in contact with the stop P. (See fig. 3.)

Now, suppose a current to pass as before in the direction indicated by the arrows. Then since, as before, T is attracted towards D and repelled from E, the tongue *will not move*, but will only

press harder against the stop P so long as the current continues to pass.

The instant, however, we interrupt the current, the polarity of D and E is reversed, D temporarily becoming a south, and E a north, pole. Hence T will now be attracted by E and repelled by D temporarily. Under the influence of this momentary reverse force, if sufficiently strong, the tongue will fly over from P to Q. But this force being essentially transient the tongue will as quickly return to its normal position.

Hence, when the relay is adjusted as here described, at the closing of the circuit there will be no motion of the tongue, but at the opening of the circuit the tongue will execute a complete oscillation, *i.e.*, will move from P to Q and back again. From the peculiar "flick" with which this movement is executed by the tongue, M. d'Arlincourt has very aptly applied to it the term "*coup de fouet.*"

It is easy to see that if we send our signalling current in the proper direction through the coils of the Relay, attach our line wire to the tongue, and "earth" or "zinc" to the stop Q, every time we release our key in sending we shall have "earth" or "zinc" momentarily put to line.

TELEGRAPH CONSTRUCTION.

To find the position of the Vertex of the Catenary, the points of support being at different levels.

By R. S. Brough.

At the outset it may be well to call to mind that in telegraph construction it is the *maximum* strain, or that at the higher point of support, which is kept constant; and that consequently the strain at the lowest point of the wire, or vertex of the catenary, is a variable. Hence we have to deal with catenaries of unknown parameters.

In practice it has been the custom to assume (1) that the curve formed by the wire is a parabola, and (2) that the strain at the insulator is equal to the strain at the lowest point of the curve. Although these two assumptions introduce no sensible error into any calculations applied to spans of ordinary length, yet in the case of very long spans the use of formulæ based on them might lead to serious mistakes.

Let a = length of the span.

w = weight of the wire per unit length.

t = the strain at the higher points of support = working strain of wire = breaking strain of wire divided by factor of safety.

$l = \frac{t}{w}$ = working "modulus" of wire = length of piece of wire whose weight is equal to working strain = ordinate of curve at higher point of support.

and k = difference of level between two points of support.

Then the above constitute our *data*.

Further, let p = strain at lowest point of curve.

$$c = \frac{p}{w} = \text{“parameter” of catenary.}$$

and x = horizontal distance of vertex of curve from the lower point of support.

Now the formula given for finding x (see Blavier, vol. ii., which, however, it is to be noted, contains numerous misprints) is

$$x = \frac{a}{2} - \frac{kl}{a}.$$

A manifest correction to this is made by introducing $(l - k)$ for l, since the lowest point of the curve must necessarily be below the lower point of support; thus:—

$$x = \frac{a}{2} - \frac{k(l-k)}{a}.$$

If we assume the curve to be a parabola, but do *not* assume $l = c$, then

$$x = a.\frac{2(a^2 + k^2 - kl) - k\sqrt{2}\{2l(l-k) - a^2\}}{2(2a^2 + k^2)}.$$

If we treat the curve rigidly as a catenary, we have first to find the parameter c from the equation

$$a = c \log_e \frac{\{l + \sqrt{l^2 - c^2}\}\{(l-k) + \sqrt{(l-k)^2 - c^2}\}}{c^2};$$

and then determine x from

$$x = c \log_e \frac{(l-k) + \sqrt{(l-k)^2 - c^2}}{c}.$$

These equations are naturally very awkward from their transcendental form, but they can be used easily enough to control our calculations.

By using the well-known approximate equation to the catenary of the form $x^2 = 2cy - \frac{1}{3}y^2$, I have arrived at the following algebraical expression, which is suitable for making the requisite calculations:—

$$x = \frac{3a(21a^2 + 25k^2 - 18kl) - 2k\sqrt{3\{24a^2(11k^2 + 18l^2) - 7(36a^4 + 27a^2kl + 4k^4)\}}}{6(21a^2 + 25k^2)}.$$

Calculations such as I here allude to have of course only to be made in office, and never on the line, and therefore the length of the above formula is no practical objection to it. With regard to its accuracy, I may state that I have compared results obtained by means of it with those given in Sir Davies Gilbert's "Table of the Ordinary Catenary," first published in the "Philosophical Transactions" (see Molesworth's "Pocket Book of Engineering Formulæ," new edit.), and in a space of 4,000 feet found them to agree within 2 per cent.

TELEGRAPH CONSTRUCTION.

Note on the calculation of the effects of Wind Pressure.

By R. S. Brough.

The force exerted by the wind during storms is so great that its effects even on such skeleton structures as overland telegraph lines cannot be neglected. This is, of course, especially the case when a number of wires have to be carried over long spans on high masts.

As regards the safety of the wire itself, since the wind pressure exerted on it will only increase directly as the diameter, while its own strength will increase directly as the square of the diameter, it is manifestly more advantageous to employ a thick wire than a thin one of the same kind and quality. Clearly, however, a thin steel wire is preferable to a thick iron one of the same strength.

If π denote the wind pressure per unit area in a direction at right angles to the axis of the wire, and d the diameter of the wire, then, practically (the wire being round), the pressure in unit length of the wire will be about $\frac{3}{4}\pi d = p$, say.

Let w be the weight of the wire per unit length. Then since gravity acts vertically downwards, while the pressure of the wind may be supposed to act horizontally, the resultant force on a unit length of the wire will be

$$R = \sqrt{p^2 + w^2}.$$

Thus the result of the wind pressure will be, so to speak, to increase the *weight* of the wire, while leaving its *strength* constant; and, consequently, to diminish the "modulus" of the wire from $\frac{T}{w}$ to $\frac{T}{\sqrt{p^2 + w^2}}$, where T denotes the breaking strain of the wire.

Considering that the utmost force of the wind may be as great as 50 lbs. on the square foot, it will be frequently found, in calculating the maximum stress, that w may be neglected against p, in which case $R = p$, nearly.

Let a = length of spans.

c = "modulus" or "parameter" of catenary.

t = original strain on wire at point of support.

and t' = new strain on wire at point of support.

Then

$$t = \left(c + \frac{a^2}{8c} + \frac{a^4}{384c^3} + \frac{a^6}{46080c^5} + \ldots\right)w;$$

and

$$t' = \left(c + \frac{a^2}{8c} + \frac{a^4}{384c^3} + \frac{a^6}{46080c^5} + \ldots\right)\sqrt{p^2 + w^2}.$$

Whence

$$t' = \sqrt{1 + \frac{p^2}{w^2}} \times t.$$

This formula shows that the wire is no more likely to break on a long span (neglecting the increased probability of flaws in the wire) than on a short one.

It shows, too, very clearly, the advantage of thick over thin wire; *e.g.* taking π as 50 lbs. on the square foot, the value of $\sqrt{1+\frac{p^2}{w^2}}$ for No. 8 wire (diam. 0″·17) is about 7·35, while for No. 1 wire (diam. 0″·30) it is about 4·24.

If d denote the "dip" of the wire, then, using the approximate equation to the catenary, the strains on the wire at the point of support may be expressed as follows:—

$$t = \left(\frac{a^2}{8d} + \frac{7d}{6}\right) w \text{ and } t' = \left(\frac{a^2}{8d} + \frac{7d}{6}\right) \sqrt{p^2 + w^2}.$$

And treating the curve formed by the wire as a parabola,

$$t = \frac{a^2 w}{8d}, \text{ and } t' = \frac{a^2 \sqrt{p^2 + w^2}}{8d}.$$

The pressure of the wind on the supports themselves is usually a comparatively insignificant quantity; but the strain on the supports arising from the pressure of the wind on the wires is often very great, increasing with the number of wires and their thickness. The magnitude of the latter can always be easily calculated, and the necessary strength to give the stays to render the supports secure can then be determined.

The pressure on each wire will be about $= p \cdot a$; and the moment of this pressure about the ground line will be $p \cdot a \cdot h$, where $h =$ height of the wire above the ground. The total pressure will be $\Sigma\,(p \cdot a)$, and the place at which to fix the stay above the ground line is given by $\bar{x} = \frac{\Sigma(p \cdot a \cdot h)}{\Sigma\,(p \cdot a)}$. The strain on the stay $= \frac{(\Sigma\, p \cdot a)}{\cos\theta}$ (where θ is the angle it makes with the horizon) $= \Sigma\,(p \cdot a) \times \frac{s}{r}$ where $r =$ the distance from foot of the post to stayhole, and $s = \sqrt{r^2 + \bar{x}^2}$.

The following table will give some idea of the force that may be exerted by the wind:—

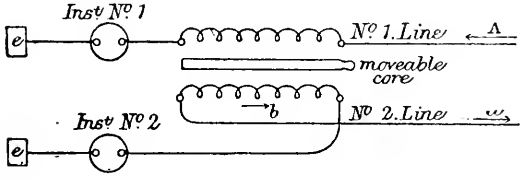
B. W. G.		Area presented by 100 yards of wire = A.	Effective area of 100 yards of wire = $\frac{3}{4}$ A.	Total pressure on 100 yards of wire when the wind force = 50 lbs. on the square inch.
No.	Diameter			
		Square feet.	Square feet.	Pounds.
1	0″·30	7·50	5·63	281
3	0″·26	6·50	4·88	244
4	0″·24	6·00	4·50	225
5½	0″·21	5·25	4·00	200
8	0″·17	4·25	3·19	160

INDUCTION BETWEEN SUSPENDED WIRES.

Arconum, 1st October, 1875.

SIR,—Mr. Culley has recently called the attention of the Society of Telegraph Engineers to the inductive effect of one wire on another, the members of the Society may therefore be interested in hearing of some experiments I made about two years ago with a view of eliminating this source of annoyance. In consequence of the gap in cable communication with the far East, between Bombay and Madras, the Government Telegraph Department found it necessary to erect two direct wires between those places. These wires are throughout the whole distance only about twelve inches apart, and they had not long been erected before it was found that signalling currents in one wire gave rise to induction currents in the other, which greatly impeded the work. It struck me at once, that these currents could be neutralised by the use of an induction coil, wound with two exactly similar wires, and connected up as shown below.

Suppose a signalling current to be sent in line No. 1, in the direction of the arrow A. At the commencement of such current there will be an induced current in wire No. 2, in the direction shown by the small arrow *a*. In the wire of the induction coil

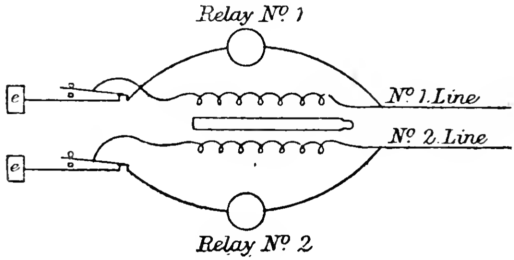


there will be generated another induced current, which will obviously be in the opposite direction to the current induced in the

line. The two induced currents can therefore be adjusted to neutralise each other.

With the permission of the superintendent of the district, experiments were made by Mr. W. P. Johnston, the then assistant-superintendent in charge of the Madras Office, and myself, in which we were perfectly successful, so far as the elimination of the induced currents was concerned, but magnetic retardation had now to be contended with. This I endeavoured to overcome by placing an electro-magnet as a shunt on the relay, as also by placing the relay in the cross-circuit of an induction bridge.

Amongst plans proposed but not carried out, the following, in



which the coil wires themselves form the shunts on the relays, may be mentioned as likely to give good results. The back contact of the key is double, one stud being connected with earth and the other with the relay. The front contact is connected with the battery in the ordinary way.

One difficulty arose from the inequality in the compensation required for sending and received currents; this is however a drawback that can be easily overcome.

Unfortunately, I was unable to complete the experiments, but, having in a few words given an idea of the principle which I endeavoured to apply, I have no doubt the method, in the hands of some of our skilful electricians in England, may be carried to a successful issue.

Yours truly,

G. K. Winter.

To the Secretary,
Society of Telegraph Engineers, London.

LIGHTNING CONDUCTORS.

DEAR SIR,—I have read with great interest the paper and discussion on Lightning Conductors in the Journal of the Society of Telegraph Engineers, vol. iv. p. 262.

I have also read the directions for the construction of conductors, issued by the French Commission on that subject. I observe, however, that almost all who discuss the subject insist most on the necessity of obtaining what Telegraph Engineers call "a good earth connection." The telegraphist uses the earth to complete his circuit, and therefore it is of great importance to him; but the protection of buildings from electric discharges has a different aim and a different method.

What we desire is, to prevent the possibility of an electric discharge occurring within a certain region, where it would be dangerous. Generally, the region we wish to protect is the inside of a building, but sometimes we also wish to render harmless any discharge between the outside of the building and other objects.

If the thing to be protected is a cottage in the country without water or gas supply, or any other system of metallic conductors extending to a distance, or if it is a ship or a balloon, then the method is simple. For we know that within a hollow closed metallic vessel no electrical disturbance can be caused by any electrical phenomenon taking place outside the vessel. For instance, Faraday built himself a cubical room of 12 feet in the side, covered it with conducting matter, and set it on insulating supports. "I went into the cube," he says, "and lived in it, and, using lighted candles, electrometers, and all other tests of electrical states, I could not find the least influence upon them, or indication of anything particular given by them, though all this time the outside of the cube was powerfully charged, and large sparks and brushes were darting off from every part of its outer surface."*

It is not necessary that the region should be entirely closed in

* Experimental Researches in Electricity, 1174.

by the metallic conductor; it is sufficient if the conductor surrounds it like a net.

Thus, in the case of a cottage which was struck last year, I placed a wire in a trench so as to surround the base of the cottage, and connected this with conductors running up the gables, and with another carried along the ridge of the roof.

If the building were a powder magazine, I would lay several stout copper wires in the foundation of the building, longitudinally and transversely, and connect them with other wires carried vertically up to the roof. These, if of copper, might be built into the wall, for security against theft. The roof should be protected by a network of wires somewhat closer than that laid in the foundation. All the lead of the roof, the rain-water pipes, and every other piece of metal on the outside, must be connected with the system of conductors. The only use of a sharp point placed on the highest part of the building is to cause any discharge to take place at that point, where it can injure nothing but a piece of copper, which can be replaced when damaged.

If, however, a building protected in this way has within it water-pipes, gas-pipes, telegraphic communication, or any other metal in communication with a large metallic system outside the building, there may be a discharge between this metal and the inside of the building.

To prevent this, it is sufficient that the system of lightning conductors should be well connected to the water-pipes, gas-pipes, &c., and, as the joints of gas-pipes are often bad conductors, all the gas-mains should be separately connected to the lightning conductor.

As it is impossible to apply this method to telegraph wires, no telegraph wires from outside should be admitted into a powder-magazine, though there may be a telegraph between different parts of the same building. It is quite unnecessary to take any precaution with respect to masses of metal entirely contained within the building, such as large machines, &c., provided they are not in communication with bodies outside, such as the water- or gas-pipes.

All that I have said depends on the fact that in order to produce an electric discharge there must be a difference of potential between

the bodies between which the discharge is to take place, and, if by putting the bodies in electric connection we can keep them at the same potential, no discharge will occur. In an isolated building, surrounded by a conductor, it is quite unnecessary to provide for the escape of the electricity to earth, for, even if it did not escape at all, it would only raise everything inside the house to a high potential, and this would do no one inside the house any harm, and would not effect even the most delicate electrometer. If the foundations were very dry, the earth discharge might, if concentrated, disturb a small portion of the soil; but it would not do any damage to the house.

In the cottage which I mentioned as being struck last summer, the chief damage was done in those parts of the walls where the mortar was still damp. Where this was near the surface the mortar only was blown out, but where it was in the middle of the wall the stones were broken, and some large granite stones forming the chimney-head were projected so as to fall seven feet from the foot of the wall.

Here the explosion was due to the conversion of the water of the mortar into steam.

In a spruce-tree struck in 1874 the upper part was wet with the thunder-shower, and was not damaged. Lower down, where the stem was dry, the electricity was conducted by the pith, and the moisture there was converted into steam, and split the tree into sectors. The sap had ascended about fifteen feet, so that the lower part of the tree was not shattered, but the bark was peeled off, and thrown to a distance from the tree. The roots extended to a wet ditch, which was the only good earth connection in the neighbourhood, and this accounts for this particular tree being struck while other trees much taller escaped.

Yours faithfully,

J. Clerk Maxwell.

Cavendish Laboratory, Cambridge.
27th May, 1876,

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THE SOCIETY

OF

TELEGRAPH ENGINEERS.

ESTABLISHED 1871.

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ESTABLISHED 1871.

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Cawood, Reuben	Postal Telegraphs, Glasgow, N.B.
Chambers, John Charles	Postal Telegraphs, Stockton-on-Tees.
Chambre, Charles F.	Manor Cottage, Putney.
Charles, G. G.	Indian Government Telegraphs.
Clapp, W. H.	14a, Austin Friars, E.C.
Clifford, W. B., M.A.	14, Maryland Road, Harrow Road, N.
Collings, Charles E.	Postal Telegraphs, Plymouth.
Collins, A.	4, Broad Sanctuary, S.W.

Collins, Lawrence . . .	Postal Telegraphs, 88, South Mall Cork.
Collins, W. C.	24, Old Jewry, E.C.
Comport, George Henry . .	Postal Telegraphs, Nottingham.
Cook, E. E.	Postal Telegraphs, 101, Cannon Street, E.C.
Cooksley, W.	Postal Telegraphs, Bristol.
Corner, C. R.	Eastern Telegraph Company, Vigo, Spain.
Cox, H. J.	48, Arthur Road, Holloway, N.
Crace, H. Winfield . . .	31, Lombard Street, E.C.
Crampton, T. Russell, M.I.C.E. .	4, Victoria Chambers.
Cromartie, Duncan . . .	Indian Government Telegraphs, Calcutta.
Cross, P. W. . . .	Postal Telegraphs, Cardiff.
Crossley, Louis J. . .	Moorside, Halifax, Yorkshire.
Cunbill, Frank W. . .	Postal Telegraphs, Plymouth.
Dallas, John	Telegraph Construction and Maintenance Company, Morden Wharf, Greenwich.
Daniel, Louis S. . . .	Government Telegraph Department, Melbourne, Australia.
Daniell, Francis T. Bristow .	Indo-European Government Telegraphs, Teheran, Persia.
DANVERS, F. C. (*Auditor.*) . . .	India Office, S.W.
Davies, William Henry .	Exchange Telegraph Company, 17, Cornhill, E.C.
Delany, Patrick . . .	Postal Telegraphs, 52, Ranelagh Road, Dublin.
Denmead, Thomas . . .	Postal Telegraphs, Exeter.
Dixon, G. H.	Postal Telegraphs, General Post Office.
Doherty, John . . .	Postal Telegraphs, Manchester.
Donald, John	Postal Telegraphs, Stranraer.
Donovan, H. C. . . .	23, Ashburnham Grove, Greenwich, S.E.
Dorman, Thomas . . .	Postal Telegraphs, Stockton-on-Tees.
Douglas, John . . .	Indian Government Telegraphs.
Draper, George . . .	Eastern Telegraph Company, 66, Old Broad Street, E.C.
Drury, William, Sen. . . .	61, South Hill Park, Hampstead.
Dunn, Andrew S. . . .	Telegraph Department, Caledonian Railway Co., Glasgow, N.B.
Durran, William . . .	Eastern Telegraph Company, Alexandria.
Eden, Augustus . . .	Postal Telegraphs, Edinburgh.
Edwards, J. R.	Postal Telegraphs, Chester.
Eggington, Alfred . . .	Eastern Telegraph Company, Otranto, Italy.

Elliott, Thomas . . .	Postal Telegraphs, Basingstoke.
Estler, Paul	Messrs. Siemens' Works, Charlton.
Evans, Frederick E. . . .	Postal Telegraphs, Birmingham.
Fahie, J. J.	Indo-European Telegraph Department, Shiraz, Persia.
Falck, A.	Great Northern Telegraph Company, Aberdeen, N.B.
Faulding, Alfred J. . . .	West Drayton.
Field, George . . .	Postal Telegraphs, Old Steine, Brighton.
Fischer, Henry . . .	Postal Telegraphs, General Post Office, E.C.
Fisher, Henry F. . . .	British Indian Extension Telegraph Company, Singapore.
Fisher, J. A., Commander R.N. .	H.M.S. "Excellent," Portsmouth.
Fleetwood, Charles Thomas .	Post Office Telegraphs, General Post Office, E.C.
Fleming, J. C.	Supt. Government Telegraphs, Perth, Western Australia.
Fletcher, John William . .	London and North Western Railway Telegraph Department, Chester.
Fletcher, Thomas . . .	Hooper's Telegraph Works, Millwall, E.
Flewell, A.	Postal Telegraphs, Bristol.
Foster, Arthur Le Neve . .	East Hill, Wandsworth, S.W.
Fraser, Arthur . . .	Verulam Club, St. James's Street.
Fraser, Charles . . .	George Town, Cape of Good Hope.
Frost, Alfred J. . . .	5, Westminster Chambers, S.W.
Gamble, D. P.	26, Coleborne Road, South Kensington.
Gavey, John	Postal Telegraphs, Bristol.
George, Edgar . . .	104, High Street, Putney.
Gibbs, Douglas . . .	Eastern Telegraph Company, Alexandria, Egypt.
Gibson, James . . .	Postal Telegraphs, Edinburgh.
Gibson, John	Kirkwall, Orkney.
Gilpin, George . . .	Postal Telegraphs, Bradford, Yorkshire.
Goodenough, Henry T. . .	4, Albert Terraee, Castle Hill, Ealing.
Goodman, H. M. . . .	Enderby's Wharf, East Greenwich.
Gosselin, George . . .	Indian Government Telegraphs, Colombo, Ceylon.
Gott, John	Freneh Atlantie Telegraph Company, St. Pierre, Newfoundland.
Granville, Arthur R. . .	26, Wellington Street, Upper Street, Islington, N.
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Grant, John	26, Old Broad Street, E.C.

Gray, Robert K. . . .	St. John's Park, Blackheath, S.E.
Groves, William . . .	89, Bolsover Street, Portland Place.
Guenzel, Ernst . . .	25, Mount Street, New Charlton, S.E.
Hall, John	Postal Telegraphs, Newcastle-on-Tyne.
Hall, John	Postal Telegraphs, Manchester.
Hall, Walter	19, Gloucester Crescent, Hyde Park.
Hamilton, F. A. . . .	Telegraph Construction Company, Enderby's Wharf, Greenwich.
HANCOCK, WALTER . . . (*Member of Council.*)	10, Upper Chadwell Street, Myddleton Square.
Harborow, Henry . . .	4, Circus Street, Marylebone, W.
Hargreaves, J. W. . . .	Postal Telegraphs, Carlisle.
Harrison, R. H. . . .	Care of R. Rookby, Ware, Herts.
Harwood, Francis . . .	Eastern Telegraph Company, Lisbon.
Hassard, H. S., Lieutenant . .	66th Regiment, Sheerness.
Haynes, J.	Postal Telegraphs, Gloucester.
Heaviside, A. W. . . .	Postal Telegraphs, Newcastle-on-Tyne.
Heaviside, O.	117, Camden Street, N.W.
Henley, George . . .	47, Essex Street, Islington, N.
Hewett, D. R., Commander R.N. .	22, Arundel Street, Strand, W.C.
Higgins, Frederick . . .	Exchange Telegraph Company, 17, Cornhill, E.C.
Higgs, Paget, LL.D. . .	Penarth, Cardiff.
Hilberd, Charles M. . . .	General Post Office.
Hill, Joseph Tripp . . .	Postal Telegraphs, General Post Office.
Hill, Thomas	Postal Telegraphs, General Post Office.
Hilliard, Augustus . . .	India Rubber Company, Silvertown.
Hoeltzer, Charles . . .	12, Queen Anne's Gate, Westminster.
Horner, Albert B. . . .	General Post Office, St. Martin's-le-Grand.
Hookey, James	Postal Telegraphs, General Post Office, E.C.
Howe, J.	Pernambuco.
Humble, George . . .	Postal Telegraphs, Newcastle-on-Tyne.
Humphries, William . . .	Postal Telegraphs, Limerick.
Hunter, J. E., Capt. R.N. . .	United Service Club, Pall Mall, S.W.
Hurley, Edward Butler . .	India Telegraph Department, Colombo, Ceylon.
Irminger, Frederick . . .	Great Northern Telegraph Company, Hong Kong, China.
Ingram, J. W.	Postal Telegraphs, General Post Office.
Isherwood, Frederick . .	Postal Telegraphs, 101, Cannon Street, E.C.

JAMES, SIDNEY	Postal Telegraphs, General Post Office, E.C.
JAMIESON, ANDREW . . .	Western and Brazilian Telegraph Company, Pernambuco.
JEFFERY, JAMES . . .	Marseilles and Barcelona Cables, Marseilles.
JENKIN, JOHN	Postal Telegraphs, Newark, Notts.
JENVEY, HENRY W. . . .	Government Telegraph Department, Melbourne, Australia.
JOBSON, HOWARD C. . . .	Dudley.
JOEL, H. F.	5, Russell Villas, Lavender Grove, Dalston.
JOHNSON, WILLIAM . . .	Postal Telegraphs, 67, Portland Crescent, Leeds.
JOHNSTONE, THOMAS Y. . .	Telegraph Department, Ispahan, Persia.
JONES, JOHN RYMER . . .	Truro House, Westbourne Park, W.
JONES, T. J.	25, Conduit Street, Regent Street, W.
JONES, J. C. D.	British Indian Extension Telegraph, Singapore.
JOSEPH, FRANK S. . . .	13, Clarewood Terrace, Brixton, S.W.
KAARSBERG, SOFUS . . .	7, Great Winchester Street Buildings, E.C.
KAISER, E. F.	6, Armstrong Terrace, New Charlton.
KEMPE, H. R.	Postal Telegraphs, Southampton.
KERRY, C. H.	Postal Telegraphs, Bristol.
KING, W. F.	Western and Brazilian Telegraph Company, Rio de Janeiro.
KNIGHT, JOHN HENRY . . .	Postal Telegraphs, Doncaster.
LAISTER, JAMES	101, Cannon Street, E.C.
LAURIE, E. H.	General Post Office.
LAVENDER, JOHN . . .	Fairy Lane, Manchester.
LAWSON, ED. G.	Entrada du Cidade 3, Funchal, Madeira.
LEE, ROBERT B.	Riband Post Manufactory, New Islington, Manchester.
LESSELS, DAVID	Postal Telegraphs, Edinburgh.
LEWIS, R. J.	General Post Office, E.C.
LLOYD, WILLIAM . . .	Whitehall Club, Parliament Street, S.W.
LOGAN, JAMES K. . . .	Inspector of Telegraphs, Dunedin, New Zealand.
LONDON, ROBERT . . .	44, Ashburnham Grove, Greenwich, S.E.
LOWE, THOMAS	8, Great Winchester Street Buildings, E.C.
LUCAS, F. R.	3, Glen Mohr Terrace, Hyde Vale, Greenwich.
LUSTY, J. W.	Postal Telegraphs, Exeter.

MACKIE, ALEX.	112, Strand, W.C.
MACKIE, S. J.	84, Kensington Palace Road, Bayswater, W.
MACLACHLAN, J. M.	Postal Telegraphs, General Post Office, E.C.
MCGAURAN, D. J.	Victoria Telegraph Department, Melbourne.
MCINTYRE, JOHN	Postal Telegraphs, Inverness.
MCKINNEY, ALEXANDER	Postal Telegraphs, Manchester.
MCLEAN, JAMES.	2, Ovington Square, S.W.
MANCE, HENRY	Indo-European Government Telegraph, Kurrachee.
MATHIESON, JAMES	India Rubber Company, Silvertown.
MAY, JOSEPH	2, Trinity Cottages, Church Street, East Greenwich.
MAYS, W.	Henley's Telegraph Works, North Woolwich.
MICHEL, R. FRANCISQUE	13, Rue de l'Ancienne Comédie, Paris.
MICKLE, DAVID	Government Telegraph Department, Melbourne.
MILLER, THOMAS	Bransford Road, St. Johns, Worcester
MÖLL, FREDERICK H. L. R.	Indo-European Telegraph Company, 16, Telegraph Street, E.C.
MOLLOY, B. C.	1, Elm Court, Temple, E.C.
MORGAN, CHARLES AMISS	Postal Telegraphs, General Post Office.
MORRIS, C.	(Scott and Co.) Hiogo, Japan.
MORRISON, M.	General Post Office.
MORRISON, G. JAMES	19, Great George Street, Westminster.
MUIRHEAD, ALEXANDER, D.Sc., F.C S.	159, Camden Road Crescent, N.W.
MUNRO, JOHN	Hooper's Telegraph Works, Millwall E.
MURPHY, CHARLES J.	Eastern Telegraph Company, Vigo, Spain.
MYGIND, JOHN	Great Northern Telegraph Company, Newcastle-on-Tyne.
NAYLOR, J. E.	Postal Telegraphs, York.
NEALE, JOHN	North Staffordshire Railway, Stoke-on-Trent.
NEWBURY, GEORGE C.	Postal Telegraphs, General Post Office, E.C.
NEWMAN, A. E.	Buenos Ayres.
NEWNHAM, W. A.	Indo-European Government Telegraph, Jask.
NEWSAM, THOMAS	Eastern Telegraph Company, Aden.
NIND, L. L.	Postal Telegraphs, Newcastle-on-Tyne.

Noble, Martin .	Postal Telegraphs, Lancaster.
Oakshot, A.	Postal Telegraphs, Southampton.
Oatway, Arthur . . .	Postal Telegraphs, General Post Office, E.C.
Ogg, F. Wm.	11, Barclay Terrace, Barclay Road, Leytonstone, Essex.
Ollard, J. F.	Lloyd's, Royal Exchange, E.C.
Oppenheimer, Joseph . . .	Manchester.
Page, Alf. S.	India Rubber Co., Silvertown.
Parker, J. E.	Western and Brazilian Telegraph Company, Bahia, Brazil.
Parkham, James . . .	Mitcham.
Parmitter, Albert . . .	Postal Telegraphs, Reading.
Parsone, E. W. . . .	175, Adelaide Road, N.W.
Partridge, G. Noble . . .	Postal Telegraphs, Exeter.
Patten, Frederick A. . .	Indo-European Government Telegraphs, Kurrachee.
Payter, J. W.	Telegraph Department, Melbourne, Victoria.
Penman, John.	West India and Panama Telegraph Company, St. Thomas, West Indies.
Petersen, K.	German Union Telegraph Company 4, Werder Strasse, Berlin.
Phillips, W. R. . . .	Paumbaum, India.
Pinchin, William Henry . .	Postal Telegraphs, Exeter.
Pomeroy, Henry . . .	Postal Telegraphs, Mullingar.
Portelli, Robert . . .	Eastern Telegraph Company Malta.
Quilley, Henry . . .	General Post Office.
Ramsay, John, Lieut. R.E. . .	Postal Telegraphs, London Bridge, S.E.
Reid, Frank	Postal Telegraphs, Newcastle-on-Tyne.
Reynell, C. H. . . .	Madeira.
Rich, H. R.	Indian Government Telegraphs, Calcutta.
Richardson, G. . . .	India Government Telegraphs, Calcutta.
Richardson, H. . . .	Postal Telegraphs, Douglas, Isle of Man.
Risch, Gustav	9, Armstrong Terrace, New Charlton, S.E.
Ritso, F. C. G.	7, Lothbury, E.C.
Roberts, Martin . . .	80, Sandringham Road, Hackney.
Robertson, J.	Postal Telegraphs, Bristol.
Robinson, F. L. . . .	Direct Spanish Telegraph Company, the Lizard, Cornwall.

ROLLS, EDWARD T.	L. and S. W. Railway, Exeter.
ROWE, THOMAS	Postal Telegraphs, Manchester.
RUSSELL, FRANK	Woodfield Road, Harrow Road, N.
SALE, Lieut. R.E.	Chatham.
SAUNDERS, H.	Eastern Telegraph Company, 66, Old Broad Street.
SAX, J.	108, Great Russell Street, W.C.
SCHAEFER, LOUIS	Imperial Telegraphs, Tokei, Japan.
SCHINDLER, ARTHUR H.	Telegraph Department, Teheran, Persia.
SCHRAMM, OTTO	12, Queen Anne's Gate, Westminster, S.W.
SHAW, JOHN	Postal Telegraphs, Bolton.
SHAW, Captain W.	73rd Regiment, Aldershot Camp.
SHEATH, ALFRED	Auckland, New Zealand.
SHEPHERD, F.	Postal Telegraphs, Brighton.
SIEMENS, ALEXANDER	12, Queen Anne's Gate, Westminster.
SIMPSON, GEORGE	Indian Government Telegraphs, Mannar, Ceylon.
SIVEWRIGHT, JAMES	Postal Telegraphs, Southampton.
SLATER, WILLIAM	Western and Brazilian Telegraph Company, Rio Janeiro.
SMITH, BENJAMIN	Eastern Telegraph Company, Malta.
SMITH, TOULMIN	12, Queen Anne's Gate, Westminster S.W.
SMITH, J. H.	6, Cornwall Terrace, Dalyell Road, Stockwell Green, S.W.
SMYTHE, JOHN	Valentia, Ireland.
SPAGNOLETTI, HYLTON	4, Circus Street, Marylebone, W.
SPARROW, W.	Putney.
SPRATT, G. O.	Eastern Telegraph Company, Porthcurno, Penzance.
STACEY, CHARLES	Eastern Telegraph Company, Aden.
STACEY, GEORGE B.	Eastern Telegraph Company, Bombay.
STEET, G. C., F.R.C.S.	Melbourne House, Rosslyn Hill, Hampstead.
STERNE, LOUIS	9, Victoria Chambers, Westminster, S.W.
STEVENSON, ED. ALF.	Telegraph Construction and Maintenance Company, Enderby's Wharf, East Greenwich, S.E.
STEVENSON, GEORGE	Eastern Telegraph Company, Alexandria, Egypt.
STEWART, D.	Postal Telegraphs, Glasgow.
STOKES, CHARLES S.	7, North Terrace, Brompton, S.W.
STOKES, HENRY L. S.	7, North Terrace, Alexander Square, S.W.
STOKES, JOHN SCOTT	R. and A. General's Office, G. P. O.

STOUT, ROBERT . . .	. Postmaster, Lerwick, Shetland Isles.
STOUT, THOMAS . .	. Postal Telegraphs, Lerwick, Shetland Isles.
STOW, G. E. . . .	. 4, Adelaide Street, West Strand, W.C.
SYMINGTON, ROBERT STEVENSON	. Glasgow.
TALBOT, FRANK F. . .	. Rosario, Argentine Republic.
TANSLEY, WILLIAM . .	. Postal Telegraphs, Portarlington.
TAPLIN, CHARLES . .	. Postal Telegraphs, Cardiff.
TARBUTT, P. F. . . .	. 11, Delahay Street, S.W.
TAYLOR, WILLIAM GRIGOR .	. Eastern Telegraph Company, Gibraltar.
THEILER, RICHARD . .	. 86, Canonbury Road, N.
THOMAS, A.	. Brazilian Submarine Telegraph Company, Pernambuco.
THOMPSON, EDWARD H. .	. 140, Leadenhall Street, E.C.
THOMPSON, H. E. . .	. Indian Government Telegraphs, Alipore.
THORNTON, M. L. E. . .	. Indian Government Telegraphs, Calcutta.
TICEHURST, F. G. . .	. Battle, Sussex.
TISLEY, S. C. . . .	. 172, Brompton Road, S.W.
TOLMÉ, JULIAN H. M.I.C.E. .	. 1, Victoria Street, Westminster, S.W.
TRENAM, EDWIN . .	. Postal Telegraphs, Leeds.
TRUMAN, EDWIN THOMAS .	. 23, Old Burlington Street, W.
TUBB, ALBERT . . .	. Postal Telegraphs, Southampton.
TUBB, H. H. . . .	. Indian Government Telegraphs, Indore, Central India.
TUCK, W. . . .	. Eastern Telegraph Company, Suez, Egypt.
TUFFIELD, T. S. . .	. 16, Anglesea Road, Woolwich.
TYLER, W. J. . . .	. 106, Cannon Street.
UREN, JOHN GEORGE . .	. Postal Telegraphs, Penzance.
VENNDT, C. F. . . .	. Great Northern Telegraph Company, Aberdeen, N.B.
VERNEY, Captain R.N. . .	. Rhianva, Bangor, North Wales.
VYLE, SAMUEL . . .	. Postal Telegraphs, Glasgow.
WALKER, WILLIAM K. . .	. Scinde, Punjaub and Delhi Railway, Lahore, India.
WALPOLE, WILLIAM BOWMAN	. 75, Patshull Road, Kentish Town, N.W.
WALTON, JOHN . . .	. Postal Telegraphs, Birmingham.
WARBURTON, E. C. . .	. Avondale, Sterne Street, Shepherd's Bush, W.
WARD, GEORGE G. . .	. Anglo-American Telegraph Company, Newfoundland.
WARREN, WILLIAM . .	. George Town, Tasmania.

WATERS, HERBERT M. . . .	9, Park Terrace, Greenwich, S.E.
WATERS, R. J., B.A. . . .	Telegraph Institution, Osnaburgh Street, N.W.
WATKINS, Lieut. R.E. . . .	Gibraltar.
WATT, GEORGE W. M. . . .	Government Telegraph Department, Mauritius.
WATSON, C., Lieut. R.E. . .	Chatham.
WEATHERALL, T. E. . . .	Telegraph Construction and Maintenance Company, Greenwich.
WEBB, E. M.	Telegraph Works, Silvertown.
WELLS, W. LEWIS . . .	Submarine Cables Trust, 66, Old Broad Street, E.C.
WERDERMANN, RICHARD . .	4, Prince's Street, Stamford Street, S.
WEST, GEORGE	Eastern Telegraph Company, Alexandria.
WIGAN, GORDON . . .	Campden House, Kensington.
WILDE, EDWIN	Postal Telegraphs, Leeds.
WILLMOT, JOSEPH . . .	Postal Telegraphs, General Post Office, E.C.
WINTER, CHARLES	Postal Telegraphs, Southampton.
WOOD, MAJOR ALEXANDER . .	Abbey Wood, Kent.
WOOLLEN, C. H. . . .	Postal Telegraphs, Exeter.
WRAY, LEONARD . .	Crosby House, Bishopsgate Street.

Total Number of Associates . . . 346

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GEE, BASIL	22, Cambridge Street, Hyde Park.
GRAHAM, WILLIAM JOHN . .	Henley's Works, North Woolwich, E.
HAYES, ALFRED	2, Westminster Chambers.
HESKETH, E. L.	Physical Laboratory, King's College, W.C.
HOOPER, SAMUEL . . .	Beechwood, Clapham Common.
KIRKMAN, JOHN P. . . .	4, Thurlow Road, Hampstead.
MCKAIN, H. F.	2, Westminster Chambers, S.W.
PALLISER, EDWARD . . .	
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PENA, JOSÉ	Physical Laboratory, King's College, W.C.
THOMPSON, GEORGE H. . .	2, Landsdowne Villas, Lee, Kent.
WARREN. J. D.	19, Pelham Street, South Kensington.

Total Number of Students . . . 15

TOTAL NUMBER OF MEMBERS.

Honorary Members	4
Foreign Members	81
Members	202
Associates	346
Students	15
Total . .	648

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